THE MATHEMATICAL GAZETTE

EDITED BY

T. A. A. BROADBENT, M.A.

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CONTENTS.

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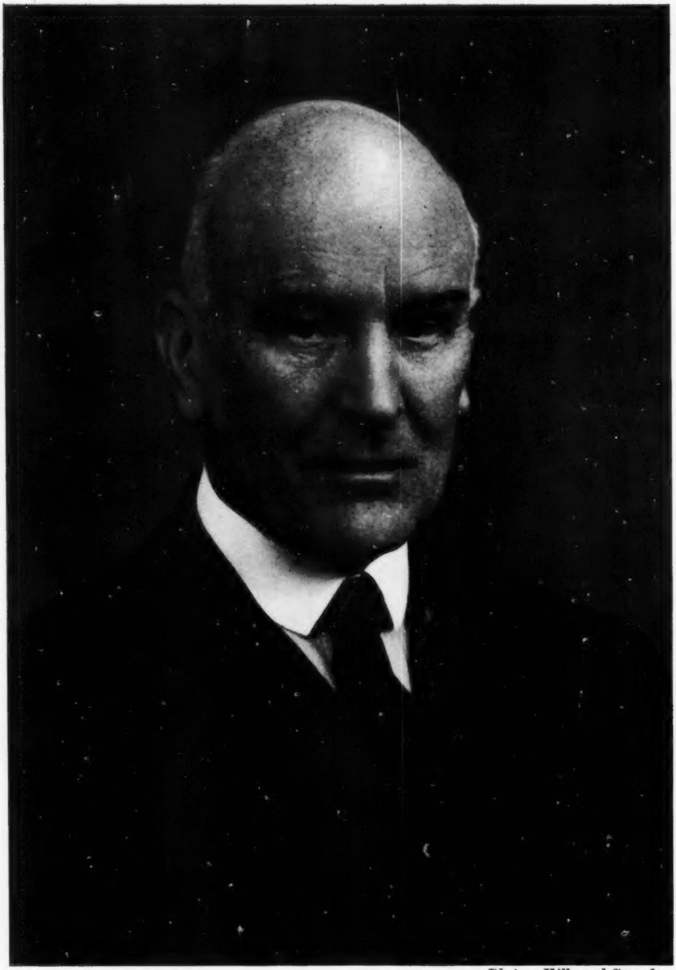

Photo: Hills and Saunders

ARTHUR WARRY SIDDONS
President, January 1935–January 1936.

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VOL. XX. FEBRUARY, 1936. No. 237

THE MATHEMATICAL ASSOCIATION.

THE Annual Meeting of the Mathematical Association was held at the Institute of Education on 2nd-3rd January, 1936. On Thursday, 2nd January, the proceedings opened at 2.15 p.m. with the transaction of business; the President, Mr. A. W. Siddons, was in the chair.

Report of the Council for 1935.* In presenting the Report the President spoke of the death of Mr. F. W. Hill and of the loss the Association had sustained through it. Mr. Hill had been Treasurer for 35 years and the Association owed him a great debt of gratitude for his care of its finances over that long period.

The Report was adopted.

Acting Treasurer's Statement for 1935. Mr. K. S. Snell, acting Treasurer since Mr. Hill's death, reported that the probable balance when the accounts were closed would be about £300. Mr. Snell informed the meeting that owing to the inconvenience of being unable to present a complete statement of accounts at the Annual Meeting, the Council had decided that in future the financial year should end on October 31st.

New Rule with reference to Junior Members. It was proposed from the chair, and carried, that the following addition be made to Rule 6, which states the conditions of membership:

Rule 6 (*b*) *Junior Members.* (i). A student in attendance at a full-time course at a University, University College, Technical College or Training College may be elected as a Junior Member for two years at an annual subscription of five shillings, provided that his membership dates from January 1st of the year in which his course is completed, or from some earlier date. His membership shall be subject in all other respects to the conditions named in Rule 6 (*a*) and he shall be entitled to the privilege named in that Rule; but he shall not be entitled to the privilege named in Rule 12 (ii).†

* See pp. 4-6.

† Rule 12 (ii) provides for the payment to a Branch by the Association of the sum of one shilling and sixpence annually on behalf of each Ordinary Member attached to that Branch.

(ii). Anyone who has taken a full-time course, as specified in Section (i) of this Rule, and applies for membership before the end of the calendar year in which his course was completed, may be elected as a Junior member for the following calendar year on payment of a subscription of five shillings. If, in addition, he wishes to join the Association for the last six months of the calendar year in which his course was completed, he may do so on payment of an additional subscription of two shillings and sixpence. His membership shall be subject in all other respects to the same conditions and he shall be entitled to the same privileges as those named in Section (i), except that he shall be entitled to receive only such copies of the *Mathematical Gazette* as may be issued during the period of his membership.

A Junior Member who, on the completion of his period of Junior Membership, wishes to become an Ordinary Member need not present himself for re-election.

The secretaries were authorised to make the consequential changes in the rules rendered necessary by the passing of the new rule.

Officers and Council. On the nomination of the Council, Prof. A. R. Forsyth, F.R.S., was elected President for the year 1936. Mr. K. S. Snell, who had acted as Treasurer since the death of Mr. F. W. Hill, was elected as Treasurer. Mr. G. L. Parsons was elected a Secretary of the Association in the place of Mr. C. Pendlebury.

Mr. C. G. Nobbs and Dr. B. Swirles retired from the Council and Professor G. B. Jeffery, F.R.S. and Miss W. M. Lehfeldt were elected to fill the vacancies.

The President proposed from the chair that, while Mr. Parsons would enter on his new duties at once, Mr. Pendlebury should be asked to remain in office until January 15th, the 50th anniversary of his election to it. This was agreed to by acclamation.

Honorary Member. In proposing Mr. Pendlebury as an Honorary Member, the President spoke warmly of his long and valuable services to the Association. He had devoted himself heart and soul to its interests and under his care it had grown and flourished wonderfully. At the Annual Meeting at which Mr. Pendlebury had been elected secretary there were present 16 members and 4 visitors, an interesting contrast to the present meeting.

Mr. Siddons spoke of the contributions that had been received to a presentation to Mr. Pendlebury; these amounted to about £32 and subscriptions were still coming in. A salver with an inscription had been bought and Mr. Siddons had hoped that Mr. Pendlebury would be there in person to receive it; but that morning he had sent a note saying that he did not feel able to attend. He had sent a message of thanks and appreciation which Mr. Siddons read.*

Mr. Siddons explained that the surplus remaining after the purchase of the salver would be handed over to a charity of Mr. Pendlebury's choosing, and that he had selected the Society of Schoolmasters.

* See p. 3.

Mr. Siddons then delivered his Presidential address: *Progress*.* This was followed by Sir Gilbert Walker's lecture, *The Physics of Sport*.†

On Friday, 3rd January, the morning was devoted to two discussions: *Rider Work in Geometry*,† opened by Mr. E. H. Lockwood and Mr. F. J. Tongue; and *Work for University Entrance Scholarships*,† opened by Mr. A. Robson, Mr. G. L. Parsons, Mr. M. H. A. Newman and Mr. A. H. Wilson.

The afternoon meeting began with a paper by Professor G. F. J. Temple, *The Rehabilitation of Differentials*,† and concluded with an exhibition of mathematical films, introduced by Miss Punnett.

A Publishers' Exhibition was open during the two days.

MR. PENDLEBURY'S MESSAGE TO THE ASSOCIATION.

I cannot in mere words express adequately how cordially I appreciate the kindly feeling and goodwill which prompted the giving to me of a memento of my 50 years of office as one of your Secretaries. I am very grateful to all who have taken part in it.

I have had a long innings, but it has been a very pleasant one to me, as I have been able to watch closely the steady growth of our membership and the progress made in the many additional forms of activity which have, from time to time, been introduced by the Council.

There is only one thing which grieves me. It is that, of the 16 members who were present at the Annual Meeting held on the 15th January, 1886, I believe that I am the sole survivor. I wish that all of them could have been here now so that they might know how well and truly they had laid the foundations of this association, and how well and truly their successors have built on those foundations and extended them. I am sure they would have been well-pleased.

But there is still one happy link between that annual meeting and this. In 1886 the President came from Harrow in the person of Mr. R. B. Hayward, who was one of the founders of this association when, in 1871, it started on its career under the name of the Association for the Improvement of Geometrical Teaching.

Now, in 1936, fifty years on, we are fortunate in having for the second time a President who comes from Harrow, and in the person of Mr. Siddons from whose continuously useful work for many years the Association has greatly benefited.

I thank you most heartily for your personal gift to me, and also for your gift to the Society of Schoolmasters, which was founded in 1798 to provide assistance for the less fortunate members of our profession and for those whom they might leave behind them. That work it has continued to do for nearly 140 years.

May I say too that I shall cherish your personal gift as an ever-present token of your kindness and goodwill and of the honour you have done me today.

* See pp. 7-26. † To be published later.

REPORT OF THE COUNCIL FOR THE YEAR 1935.

During the year 1935, 121 new members have been admitted to the Association. The number of members now on the roll is 1485, of whom 8 are Honorary members, 99 are life members by composition, 4 are life members under the old rule and 1374 are ordinary members.

The Council regrets to have to report the deaths of the following members of the Association: Rev. W. R. Blenkinsopp, Dr. H. E. J. Curzon, Major E. T. Dixon, Mr. F. W. Hill, Dr. E. M. Horsburgh, Mr. J. P. Kirkman, Mr. T. E. Monckton, Emeritus Professor J. E. A. Steggall, and Mr. Harry Thomas.

Mr. Hill had been Honorary Treasurer of the Association since 1900. During this long period of more than 35 years, Mr. Hill's services to the Association in watching over its financial welfare have been very great. His pleasant personality will not soon be forgotten.

Professor Steggall had been a member of the Association for 55 years since 1881. On his retirement in 1933 from his Professorial Chair at University College, Dundee, in the University of St. Andrews, after 50 years' service, he received the title of Emeritus Professor. He was often present at the Annual Meetings of the Association.

The Mathematical Gazette.

In the July *Gazette* a French text-book on Geometry was the subject of a detailed review by Mr. C. O. Tuckey, written in conjunction with an article in the same number by Professor S. Minois of Brest, entitled "L'Enseignement des Mathématiques en France dans les Lycées et Collèges." It is hoped by further use of this method to keep readers of the *Gazette* in touch with teaching ideals and practice in other countries.

The Branches.

The formation of the Branches Committee has brought about a much closer relationship between all the Branches and much benefit has been derived from it by the Association as well as by the Branches themselves.

All the Branches report a successful session and useful meetings. The Algebra Report, issued in 1934, has been discussed at many meetings. Particulars of the meetings have been given in the *Gazette* insets. Some of the Papers read before the Branches have been published in the *Gazette*, and the Yorkshire Branch has again issued a full independent Report of its proceedings. The Branches in Wales appear to be particularly active.

A Branch has recently been formed in Northern Ireland and another is in process of formation in New Zealand.

The composition of the Branches in terms of members and associates is as follows: London, 191 members, 112 associates; Yorkshire, 48, 90; Manchester and District, 38, 70; Bristol, 12, 19; Midland, 28, 35; Liverpool, 21, 59; North Wales, 4, 12;

South-West Wales, 10, 42 ; North-Eastern, 41, 16 ; Southampton and District, 2, 22 ; Cardiff, 27, 42 ; Queensland, 11, 14 ; Sydney, New South Wales, 19, 122 ; Victoria, 7, 8.

The Library.

An uneventful year has enabled the Librarian to prepare a list of accessions since 1929, and this, completed in the press to the end of 1935, will be issued early in 1936.

The General Teaching Committee.

The General Teaching Committee has begun to discuss the work of mathematical specialists, particularly in their first year after the school certificate. A sub-committee has been appointed to prepare a new list of books suitable for school libraries: the work of this sub-committee is now almost completed.

The Boys' Schools Committee.

The Boys' Schools' Committee during the year has continued its work on the supplementary Geometry Report. This report is now taking definite shape and it is hoped that it may be completed by the autumn of 1936.

The Girls' Schools Committee.

The Girls' Schools' Committee has met twice during the year 1935. Reports were read by various members and discussion followed on the investigation of interesting side-lines in several branches of mathematics.

The Problem Bureau.

There has been no decline in the number of applications. Many of these have come from the large number of new members who were elected about a year ago.

As the Bureau has now been in existence for about seven years, the following statistics may be of interest :—The number of members who have sent problems exceeds 100 : the total number of applications is about 225 and the total number of solutions supplied about 350.

The secretary of the Bureau is very grateful for the valuable help he continues to receive from the staff of solvers, whose number has been recently increased.

Officers and the Council.

Mr. Pendlebury has expressed a wish to resign from the office of honorary secretary. He was elected secretary of the Association for the Improvement of Geometrical Teaching in January 1886, so that he has been secretary for exactly 50 years. At the present time there are only three members of the Association who joined before Mr. Pendlebury became secretary. Under his care the Association has extended its interests from those of the A.I.G.T. to the wider

interests of the Mathematical Association, and its membership has increased tenfold and extends to the furthest parts of the Empire. The Association owes a very great debt to Mr. Pendlebury for the splendid work he has done for it.

Since Mr. Hill's death, Mr. K. S. Snell of Harrow School has acted as treasurer.

The period during which a President may remain in office having been reduced to one year, Mr. Siddons now retires. He has had for many years a close personal connection with the Association, from which it has benefited in many ways. The Council desires now to express its most cordial appreciation of the valuable service which Mr. Siddons has rendered to the Association not merely during his year of office but for many years before.

The Council nominates Professor A. R. Forsyth for election as President of the Association for the year 1936.

The Council desires also again to express its thanks to Mr. T. A. A. Broadbent, Professor E. H. Neville and Mr. A. S. Gosset Tanner for the great services which they continue to render to the Association as Editor, Librarian and Director of the Problem Bureau respectively; and to Dr. Bertha Swirles and Mr. C. G. Nobbs for their co-operation as members of the Council since 1932.

ADDRESS OF THE NEW SECRETARY.

COMMUNICATIONS for Mr. G. L. Parsons should be addressed to Peckwater, Eastcote Road, Pinner, Middlesex.

GLEANINGS FAR AND NEAR.

1034. The ideal of mathematics should be to erect a calculus to facilitate reasoning in connection with every province of thought, or of external experience, in which the succession of thoughts, or of events, can be definitely ascertained and precisely stated. So that all serious thought which is not philosophy, or inductive reasoning, or imaginative literature shall be mathematics developed by means of a calculus.—A. N. Whitehead, *Universal Algebra*, p. viii. [Per Prof. H. G. Forder.]

1035. Notre conversation prit un tour philosophique. Je n'étais pas fort en ce domaine, mais j'en avais le goût, et de même mon pâle voisin. Il poursuivait un double rêve qu'il me confia, tout à la fois métaphysique et mathématique. Il prit un papier et, pour se faire mieux comprendre, m'écrivit quelques équations qu'il avait établies : son ambitieux désir, qu'il me communiqua, était de donner une forme algébrique à son idée de Dieu, de résumer l'univers en quelques signes, et enfin de résoudre ces signes par le signe *infini*. Mais il était arrêté par des difficultés techniques, il me l'avoua avec tristesse.—Daniel Halévy, *Pays parisiens*, p. 166. [Per Mr. J. B. Bretherton.]

1036. That, and that alone, is why the Plafond enthusiasts and Auction diehards are beginning to make a stand against Contract. It is not that we desire to be retrogressive, but an honest desire to keep Bridge as a game and not a form of study that makes the Integral Calculus look like child's play.—Letter to the *Times*, Sept. 22, 1934. [Per Mr. J. Clemow.]

PROGRESS.

By A. W. Siddons.

Presidential Address to the Mathematical Association, January 1936.

First of all I must thank you for the great honour you have conferred on me in electing me to be your president. I feel the honour is especially great because it is the first time that the Mathematical Association has elected a mere assistant master to the chair. The A.I.G.T. had one, and only one, assistant master president. Of him I shall speak later.

Professor Neville's remarks last year about the progress, or lack of progress, of the scholarship boy set me thinking about the general progress that has been made in mathematical teaching and about the extent to which this Association is responsible for that progress. That not unnaturally made me look at the conditions that existed in 1871 when the A.I.G.T. was founded. My investigations caused me to look once more at that most interesting report of the Royal Commission (of 1861), on certain Public Schools;* there I found some account of the introduction of mathematical teaching into Public Schools; and I thought you might be interested, if I gave you to-day some account of the work of the last century.

You must forgive me if Harrow looms rather large in what I say. For obvious reasons I have had more opportunity of finding out about teaching at Harrow, and Harrow was the scene of very many of the committee and sub-committee meetings in the days of the A.I.G.T., and lastly the report of the Royal Commission of 1861 happens to give more account of the mathematical teaching at Harrow than of that at most other schools.

Early Days of Mathematical Teaching.

Mathematics were introduced into the ordinary work at *Merchant Taylors'* in 1828, at *St. Paul's* in 1842—Mr. Pendlebury tells me that before Walker's time at St. Paul's (he became Head Master in 1877), the highest mathematical work was done by a visiting master, who went there on two afternoons a week; at *Westminster* mathematics were introduced in 1828; there was no mathematical master on the staff till 1846, but boys going to College had some instruction from classical masters—no mathematics were taught as a special branch.

Arithmetic became part of the instruction given at *Rugby* in 1780. "Between 1820 and 1830 there were two mathematical masters who were not invested with any authority. The hours spent in the writing school were too often hours of idleness and confusion with many boys and must have been, therefore, also hours of disappointment or of nonchalance with those appointed to teach them."

Dr. Arnold, soon after his appointment (1828), placed the teaching of the higher branches of arithmetic and mathematics in the hands

* Published in 1864.

of the classical masters, Dr. Tait in 1842-1847 appointed two efficient mathematical masters.

At *Eton* before 1836 there appears to have been no mathematical teaching of any kind, but a Mr. Hexter had been there as teacher of writing and arithmetic only.

In 1836 Mr. Stephen Hawtrey (11th Wrangler) went to Eton; " but, in order not to trench on the interests of Mr. Hexter, the only boys allowed to learn of Mr. Hawtrey were those of the Headmaster's division, which contained about 30 boys, or any others who had obtained a certificate from Mr. Hexter that they had attended his classes and were competent to attend Mr. Hawtrey. This arrangement did not prove satisfactory to Mr. Hawtrey; and after about three years Mr. Hawtrey was allowed by the authorities of the College to disembarrass himself and the school of Mr. Hexter by undertaking to pay him a pension of £200 a year. A deed to this effect was executed, with the concurrence of the Provost and Fellows, by the two contracting parties, and Mr. Hexter resigned ".

No place was provided for Mr. Hawtrey to teach, so he leased a plot of ground from the College and built, at considerable expense to himself, a mathematical school (referred to in the report of the Public Schools Commission as a " theatre "). Apparently he expected that, when he retired, his successor would buy it from him.

From 1836 to 1851 Mr. Hawtrey merely gave private lessons to such boys as wanted them and had no salary from the College, but—

" In 1851 mathematics were for the first time incorporated into the regular work of the school and Mr. Hawtrey was made Mathematical Assistant Master, which placed him on the same level as Classical Assistants. His own Assistants, however, did not share his elevation, they became ' Assistants in the Mathematical School ', which position they still occupied in 1861."

At first they were not allowed to wear Academic dress, but later they were allowed to wear it in School, but not in Chapel; in 1861 they were allowed to wear it in Chapel. " But they have no authority out of school, and therefore are not felt to be real Masters, by the boys."

At *Harrow*, mathematical teaching seems to have begun really in 1819, and it was first made compulsory in 1837.

" Before 1819 mathematical instruction could only be obtained from a Writing Master (who was then very old), except that boys in the VIth form read Euclid once a week with the Head Master, a practice introduced by Dr. George Butler (Head Master 1805-1829), who had been Senior Wrangler and Smith's Prizeman."

In 1819 J. F. Marillier was appointed—he is described in the list of masters as " Writing and Mathematics "—and he gave private lessons to such boys as desired it, until in 1837 mathematics was first made compulsory.

From 1837 to 1857 there were 2 mathematical masters; from 1857 to 1859 there were 3; since 1859 there have been 4 or more.

Between 1837 and 1857 the number of boys in the school varied from 60 to 460.

Of early mathematical masters, I will mention Colenso (1838-1842), whose name will be remembered in connection with his arithmetic and algebra books. He had been Second Wrangler and was afterwards Bishop of Natal and, I presume, gave his name to the place which was the scene of the battle in the South African War.

Of Marillier, I must speak too ; there were two Marilliers, one who taught " Foreign Languages ", who came to Harrow about 1819 and left in 1839, and J. F. Marillier, whom I mentioned before as " Writing and Mathematics " ; he stayed from 1819 to 1869. There is an old house in the High Street at Harrow which is still called " Marillier's ". He was a Frenchman, of course, and always spoke of " arithm*étique* " and " mathéma*tique* " ; consequently at Harrow arithmetic and mathematics are still called " Tique ". The word has a double meaning : a boy speaks to-day of going up to " Tique ", meaning that he is going to a mathematical lesson ; a mathematical master is a " Tique Beak " ; inside the mathematical master's room, the boy might say, " Are we doing Tique or Algebra or Geometry ? " —in that case the word " Tique " of course means " arithmetic ". Marillier himself was nicknamed " Tique " or " Teek ".

An old Harrovian, Mr. R. Courtenay Welch, who is still a very active Army Coach, writes to me : " I went to Harrow on 25th April, 1864, being then 12½ years old. I joined after the term had begun and so missed the ordinary Entrance Examination and was examined by my tutor ' Skipper ' Holmes. I knew no Greek and little Latin and so was placed in the Third Fourth and was ' Lag ' of the School. But I knew some Mathematics, practically all Arithmetic, Algebra . . . Quadratic Equations, Four books of Euclid of which I was very fond and a little Trigonometry.

" However, Mathematics in the Third and Third Fourth Forms (which worked together in all subjects) were taught by Mr. Marillier (' Teek ') . . . . Nearly half a century before he had joined the School as Writing Master and his mathematical knowledge did not extend much beyond the Rule of Three. I think we only went to him twice a week, when we did little else but work out these sums. The point on which he laid the greatest stress was that we should write against each sum a large I or D indicating Indirect or Direct Proportion."

Another mathematical master of the last century was R. B. Hayward (1859-1893) ; he was an F.R.S., and is one of the few school masters who have examined in the Mathematical Tripos ; he was President of the A.I.G.T. from 1878 to 1888. He is the only other assistant master who has ever been President of the A.I.G.T. or the M.A., so you will understand how honoured I feel to be occupying the chair to-day : it is of interest to me that the first two assistant master presidents have both been senior mathematical masters at Harrow.

I once spent a long evening with Hayward talking over mathe-

matical teaching, and, in particular, the early work of the A.I.G.T., in which he had played a very important part, but of that I will speak later.

I will quote again from Mr. Courtenay Welch's letter:

"Two terms later, when I got into the Fourth Shell (in 1865) I worked with Hayward ('Haycock') and remained with him for the rest of my school life .... Then the forms were divided up for Mathematics and I think there were only 16 in each division, the other half going to French. Hayward was a first-rate teacher for those who had any mathematical ability, but could not descend to the level of the average boy. We spent—in the lower forms—a great part of our time in saying and writing out Euclid; but Geometry remained a mystery to many, for very little explanation was ever given."

I am particularly glad to be able to quote that Hayward was a first-rate teacher for the able boy, as the stories that live at Harrow are mainly how the average boys enjoyed themselves up to him; he had a trick of saying "Silence" or even "Silence, Silence, Silence" —little did he realize that before they went into his room, sides had been picked up and that every "Silence" counted as one run, and the side was put out when he set a punishment. Even if he could not deal with the non-mathematician, he had excellent ideas on teaching and he was a great teacher of those who were mathematically minded.

Now let me give you some further facts gleaned from the report of the Royal Commission of 1861.

Time Devoted to Mathematics.

"The number of school hours in the week assigned to arithmetic and mathematics at Eton, Harrow, Rugby and Shrewsbury, taking one form with another is three; at Westminster and St. Paul's, four; at the Charterhouse, five; at Winchester, seven or eight in the upper part of the school and three in the lower; at Merchant Taylors', ten. At Winchester, however, and probably at Merchant Taylors', the lessons are prepared as well as done in schools. At the schools where this is not the practice, each lesson is supposed to require about an hour of preparation."

The number of mathematical masters varied from 1 for 60 boys up to 1 for 150 (Harrow 1 for 120).

The report tells us that Mathematics did not affect a boy's place in the school at St. Paul's and Westminster, but they did at Eton, Winchester, Harrow, Rugby, Shrewsbury and Charterhouse—at Harrow they were given one-quarter of the weight given to classics.

Standard of Mathematics in 1861.

After mentioning that the chief honours and prizes at schools are given for classics, the report of the Commission says:

"Classics are, to the great majority of boys, intrinsically more

attractive than mathematics.... But mathematics at least have established a title to respect as an instrument of mental discipline; they are recognised and honoured at the Universities, and it is easy to obtain Mathematical Masters of high ability who have had a University education."

The report goes on to say that the mathematical teaching at the Public Schools is not as efficient as at the Grammar Schools. Several University tutors gave evidence as to the attainments of candidates for matriculation. The Senior Censor of Christ Church says: "Their answers in arithmetic do not encourage us to examine them in Euclid or Algebra."

The Junior Censor says: "A couple of plays of Euripides, a little Virgil, two books of Euclid, or the like, form the occupation of a large part of our men during their first University year."

But the evidence from some schools at any rate gives us a brighter impression.

At Rugby, "an average boy leaving the school at 18 will have gone through arithmetic, algebra to the end of progressions, and the first four books of Euclid, without extra tuition. By private tuition a few boys are enabled to understand the differential calculus before quitting the school."

At Eton. "The mathematical reading of an average boy extends to the first part of Colenso's *Algebra* and four books of Euclid. A fair number read Trigonometry; a few advance to conic sections (presumably geometrical) and fewer to analytical geometry, which is the highest point. Mr. Hawtrey has never taken a boy into the Differential Calculus."

At Harrow from 1837 to 1845 every boy did 2 hours of Mathematics a week, but none of the boys were reading beyond Arithmetic and a very little Algebra and Euclid.

In 1861 the lowest form did Mathematics for 2 hours a week, the rest for 3 hours a week and preparation usually occupied them for 2 to 3 hours per week more. Boys were taught in small divisions, averaging about 18 or less. Besides this about 40 boys took private lessons in Mathematics.

In 1861 Mr. Watson computed that nearly half the boys who leave the sixth form have gone through six books of Euclid; one-third through Trigonometry; two-thirds may have a very fair knowledge of Algebra, including Quadratic Equations. Probably one-third leave with very little knowledge of Algebra; they take a much greater interest in Arithmetic. Mr. Middlemist said that in the VI "there are a small number in Conic Sections and Mechanics; and in private tuition we have some in the Differential Calculus."

A boy who left Harrow in 1859 (R. Lang) gave evidence. His own private work was in mathematics. In mathematics he got to conic sections and statics, and in private tuition, to the differential calculus; there were facilities for learning as much as a boy wished; the mathematical masters took great pains with the boys.

He did not at Cambridge know as much mathematics as the best scholars from other schools; about as much as the pupils from the larger Public Schools, but in all the newer schools the pupils were better prepared in mathematics, though not usually so in Classics.

I cannot find his name in the Mathematical or Classical Tripos list, but he was in the University Cricket XI for 3 years.

Then I have Mr. Courtenay Welch's statement of what he did between 1864 and 1871; he read Algebra, Euclid, Trigonometry, Conic Sections (Geometrical and Analytical) and Calculus (possibly only Differential, at any rate there were no examination papers on Integral Calculus).

Dr. Gerald Rendall (afterwards Head Master of Charterhouse) recently wrote to me, giving his recollections of mathematics at Harrow, between 1864 and 1870. He was really a classic, but seems to have been particularly well grounded in mathematics at his preparatory school; mathematics with him were always subsidiary to classics, but he did the regulation three hours a week of mathematics through all his time at Harrow and won the highest mathematical prize. He speaks of his work in trigonometry, mechanics and conic sections, but he did no calculus—in his last year he only kept up the mathematics he had already read as his time was much taken up with prize exercises and competitions.

From what I have quoted to you and from other brief statements in the Public Schools report, I gather that in 1861 the few best mathematicians in various schools did trigonometry, mechanics, conic sections and in some schools differential calculus; but the majority of boys seem to have done nothing beyond about four books of Euclid, some Algebra and Arithmetic, and the work was generally very mechanical.

Now I come to

The Birth of the A.I.G.T.

In his Presidential Address in 1921, Canon J. M. Wilson told us that the publication in 1868 of the report of the "Schools Inquiry Commission"* made it plain that time was being wasted over Euclid: that boys might have worked for years at Euclid, and even know Euclid perfectly, and yet know next to nothing of the spirit or method or the results of Geometry. He explained that agitation was started: meetings were held and letters were written to the *Educational Times*. In 1868 Wilson himself produced a Geometry book.

In the second volume of *Nature* on 26th May, 1870, a letter appeared from Rawdon Levett proposing the formation of an Anti-

* This is a later Commission than the one whose report I have been quoting so freely. Here are two quotations from the 1868 report:

"The teaching of mathematics in English schools is rarely satisfactory."

"It seems evident that while the teachers, as a rule, do not take much interest in the subject, in all probability the methods of teaching also want improvement."

Euclid Association. A good deal of correspondence took place, some of which Levett showed me thirty years afterwards, and in October 1870 a meeting was called for the following January * and a list of members was published—I have here a copy of that list of October 1870. I believe it to be the earliest document of the A.I.G.T., it contained 28 names, of which 24 were schoolmasters. With the report of the first meeting of January 1871 a new list of members appeared which contained 61 names, of which 52 were schoolmasters.

Quite definitely the Association was an association of schoolmasters founded to benefit school teaching; University professors were drawn in, partly no doubt, to help to keep the Association on right lines, but mainly to help in changing examination regulations that were strangling school teaching.

Rawdon Levett.

Of Levett I should like to say more, for he was the real father of this Association, and, as secretary of the A.I.G.T., for its first 13 years nursed the babe with the utmost care and energy. He was a very shy and retiring man and the great work that he did for the A.I.G.T. was done in such a quiet unassuming way that he has not had the full credit that he deserved. I only learnt how much he had done from a classical colleague of his who shared his bachelor home, and from old members of the A.I.G.T. like Canon Wilson, R. B. Hayward and Done Bushell, and from the masses of correspondence which I went through with him about 1902 before he destroyed most of it. Luckily I rescued some of the papers and I hope to go through them again in the course of the coming year, and hand them over to the library; they include several early drafts of the A.I.G.T. syllabus, a circular by the late Archbishop Temple on Parallel Lines, a paper by the late Lord Moulton on Ratio and Proportion.

I gather from the 10th General Report of the A.I.G.T.† that the A.I.G.T. syllabus and the Geometry were very largely the work of Levett and Hayward,‡ and talking with Hayward about 1902 I found confirmation of this, at any rate so far as Levett was concerned.

I first met Levett in 1890; it was his unconscious influence that made me think of becoming a schoolmaster. At the time I was first "up to" him I had been learning Euclid and Algebra for one term; I have no particularly vivid recollection of his Geometry lessons, but in Algebra he made a great impression on me. I had already done the four rules and some manipulation, very blindly; he introduced

* A copy of the notice calling the meeting was published in *Nature* for 29th December, 1870. It was signed by Levett, MacCarthy, Wilson and Tucker.

† p. 40. The President said: "If the Association had attained any success through one individual more than another, that individual was Mr. Levett. . . . he had been indefatigable in his work for the Association, and in particular with regard to the Plane Geometry Committee. Whatever merit their book might possess was owing to a very great extent, to Mr. Levett."

‡ This is explicitly stated so far as the syllabus is concerned, on p. 24 of the 15th report (1889).

me to problems and there was a new light. I remember that he was particularly careful about the definition of the unknown and required a very full statement: one day he remarked: "By the time you have defined x you have repeated the whole question."

I was only up to him for two terms then, but the tall, thin, shy man made a great impression on me. I was not up to him again until two years before I left school, when I suddenly made up my mind to give up trying for a classical scholarship and to take mathematics instead; at that time I went into the top mathematical division, which he took, and then I saw a great deal of him and grew to know him well, both in school and out.

Levett had had an interesting career before he started teaching: he was educated at Pocklington Grammar School and St. John's College, Cambridge. His father died a few weeks before his tripos and he had to go down and settle his affairs, but he was 11th Wrangler. He stayed on at Cambridge to coach his younger brother, Ernest, who was 3rd Wrangler and afterwards a well-known barrister.

I am not sure whether his first teaching post was at King Edward's School, Birmingham, but he certainly went there soon after leaving Cambridge. The standard of work there was very high and it had a great reputation for producing classical scholars, just as later it had for winning mathematical scholarships.

Levett was a great teacher and a great organiser: in his quiet way he had a great influence on his mathematical colleagues and at first he had to teach some of his classical colleagues to help with the mathematical teaching.

As a teacher he must have been rather unique: he made us think for ourselves and had wonderful discretion in leaving us to fight out our own battles. The one thing he abhorred was cramming: he never let a boy sit down to a scholarship paper. If ever a boy ventured to ask whether the work in hand was of any help for his scholarship examination, Levett's invariable reply would be: "It is not my business to win scholarships for you, I have to make you love beautiful series." I remember one incident particularly. We used to have one subject a day and on Wednesdays it was always Analytical Geometry; on one Wednesday he called two of us up to his desk and asked how we had got on; we were reading Salmon's *Conics*, and we said that we had read four lines, "What is the difficulty?" he said. For ten minutes the other boy and I argued about those four lines without coming to any agreement and without a word from Levett; at the end of that time he merely said: "Very nice", and called up the next group of boys.

With our ordinary work we went on quietly but he always kept some outside subject going; sometimes it would be Astronomy—eclipses, the Harvest Moon, or the precession of the equinoxes. I remember vividly a term in which we worked at falling chains and eventually solved problems by differential equations—I remember still that he never gave us any rules for solving them, and terms later,

when at Cambridge I had learnt more about differential equations, I asked him why he had not given us one particular rule that would have been so helpful, he merely replied : " It was so good for you to flounder."

He was particularly interested in the fundamentals of algebra and the convergence of series ; in the preface of the second edition of Chrystal's *Algebra*, you will find that Chrystal thanks Levett (wrongly described as " of Manchester " instead of as " of Birmingham ") for acute criticism which caused him to recast and greatly improve some chapters.

We had our weekly problem paper which always consisted of seven problems from recent scholarship papers—we had to copy them from his manuscript. He took infinite trouble over our solutions : I remember on one occasion I showed up an attempt that ran to several pages and was given up in despair ; Levett wrote out and brought up two or three pages more which finished off my attempt and also his own solution of the problem which took half a page : the pains he had taken to finish my solution made a great impression on me.

One morning in May 1894 we were all sitting hard at work, and directly Levett came into the room he walked round and gave each of us a copy of the first number of a new periodical called *The Mathematical Gazette*. To-day I am the proud possessor of a complete set of *Mathematical Gazettes*.

Before I finish talking of Levett, I must quote from one of his letters written in 1902. The Teaching Committee of the M.A. was working at arithmetic and I wrote and asked Levett's opinion about two or three points which were subjects of heated discussion.

After speaking of other points he goes on :

" I care not a rush whether boys use an x or not in working an arithmetical problem ; if it helps them to think then good, if it is a symbol used mechanically then bad, very bad.

I trust that in the new reforms you will aim at giving freedom to the teacher ; my present position with regard to reform might perhaps be summed up in the following resolutions :

1. That it is desirable that every schoolmaster should be intelligent, zealous and stimulating.

2. That good schoolmasters should be well paid in cash and in repute, and that bad ones should be requested to find other occupation.

3. That a good schoolmaster should be allowed to do what he liked.

4. That examiners should go to the devil."

I am glad to have this opportunity of paying in public a tribute to Rawdon Levett, but I must return to the early days of the A.I.G.T.

I have already said that in his Presidential Address of 1921, Canon J. M. Wilson told us that in 1870 the learning of Euclid

occupied most of the time the ordinary boy devoted to Geometry; the A.I.G.T. was founded to get away from this tyranny. What was its success in this? At first sight the answer is "practically nil". Thirty years after, when I started teaching, the learning of Euclid still held the dominant place. But, when we look deeper, it is clear that the spade work that was begun in 1871 produced some result in the last century, but its real harvest has been reaped in this century. In my opinion, it is entirely due to the work done by the A.I.G.T. in the first 17 years of its existence that we were able, in the early days of the present century, so quickly and so unexpectedly to sweep away the tyranny of Euclid.

What did the A.I.G.T. actually do? First of all it stirred up much discussion and circulated its reports, which gave much admirable advice and thereby did much to improve the geometrical teaching in the country. In 1888 Professor Chrystal wrote: "I think your Association has already done much good to the teaching of Geometry. I begin to find intelligent teachers of Geometry in a good many places now, where formerly no such thing could be found."

It has been of considerable interest to me to read through the early reports of the A.I.G.T. and at the same time to look at the Euclid papers set at Harrow about the same time. Remembering that Hayward, particularly, and Bushell and Marshall were active members of the A.I.G.T., it is not unnatural to find recommendations of the A.I.G.T. reflected in the Harrow papers and no doubt in those of other schools, though I have not had the chance of verifying this.

The Harrow Geometry papers of 1870 were very long, 20 to 24 questions, ranging from Euclid, Book XI to Book I, with a few riders, very few.

Soon we see the demand for riders:

(1) Limitation to writing out not more than 9 propositions, but as many riders as the boy liked.

(2) Then on the modern side two papers, one on propositions and one on riders.

Apart from the stirring up of the teachers of the country, the more tangible work of the A.I.G.T. consisted of the production of an admirable syllabus and textbook of geometry. The trouble taken over these was enormous: in the first instance nine syllabuses were drawn up by different people, these were considered by the committee, who reported:

"In these syllabuses we remark entire coincidence in the following points:

(*a*) That Practical Geometry should precede Theoretical.

(*b*) That numerical examples should be introduced in illustration.

(*c*) That the axiom known as Playfair's respecting parallels should be substituted for Euclid's.

(*d*) That the notion of rotation should be introduced into the definition of an angle.

(*e*) That Euclid's treatment of regular figures in Book IV is unsatisfactory.

(*f*) That the treatment of proportion for both commensurable and incommensurable magnitudes should be rigorous.

(*g*) That Problems should be separated from Theorems.

(*h*) That the conception of a Locus should be introduced.

On the following points there is great, though not entire, agreement:

(α) That it should be considered as an axiom that the shortest distance between any two points is the straight line joining them.

(β) That the treatment of parallels should precede that of the comparison of triangles.

(γ) That the restriction of the term "angle" to such angles as are less than two right angles should not be maintained.

(δ) That the method of superposition should be more freely used than in Euclid.

(ϵ) That in the treatment of a tangent the method of limits should be recognized from the outset.

(ζ) That Euclid, III, 35-37, should be proved by similar triangles."

These points of agreement and twenty resolutions arising out of them were printed with very wide margins and submitted to the annual meeting of January 1872. A sub-committee then drew up a syllabus that covered the ground of Euclid, Books I-IV; this was revised and revised and circulated to members in December 1872. Certain points in this were altered at the annual meeting of January 1873, and it was then sent to the Committee of the British Association. I have copies of parts of the syllabus in some of the preliminary stages, they were printed with margins wider than the text, so that the sub-committee were obviously prepared for long criticisms and suggestions.

Ratio and Proportion took much longer to deal with and produced many interesting printed documents, of which I have copies, including treatises on the subject by Fletcher Moulton, Hayward, Levett and others.

Eventually the syllabus was published in 1875 after more than four years' work.

In January 1881 it was decided to produce the textbook and this was published in 1884. Also in January 1881, committees were appointed to report on Solid Geometry, Higher Plane Geometry and Geometrical Conics.

Meanwhile the committee were perpetually trying to influence examining bodies but without great success; Oxford and Cambridge moved very slowly. But "the questions in Geometry proposed at the Matriculation Examination of the University of London in June 1876, deviated from the old Euclidean type so far as to provoke a spirited controversy in the columns of the *Times*".

I note from the A.I.G.T. reports that:

In 1874 in the pass part of the Oxford and Cambridge Joint Board's Certificate, there was not a single geometrical exercise.

At the meeting in January 1878, several speakers pointed out that " bookwork alone is quite enough to pass any existing examination " and a resolution was passed urging that rider work should be essential for passing an examination in Geometry.

In 1887, after 16 years' work, they are still bombarding the Universities.

I remember Levett telling me that on one occasion he and others went up to Cambridge to a meeting of the Mathematical Board at which they hoped the Board would agree to the abolition of Euclid. I think this meeting must have been in 1887. Cayley was in the Chair and dominated the meeting. One remark of Cayley's Levett quoted to me, " the proper way to learn geometry is to start with the geometry of n dimensions and then come down to the particular cases of 2 and 3 dimensions ". Cayley opposed the abolition of Euclid and Levett told me that after the meeting a member of the board said to him, " We cannot go against Cayley ".

The Fifteenth General Report (January 1889) mentions the debate when the suggestion to allow proofs other than Euclid was discussed by the Senate. Mr. E. M. Langley speaks about the debate.

" One or two uncompromising opponents had been met with. Professor Cayley was the most formidable ; and he was such an ardent admirer of Euclid that he overshot the mark, and his opposition told in favour of the Association. In the course of the discussion he (Mr. Langley) having drawn attention to the fact that the authorised treatise was an inconsistent one—a mixture of Euclid and Simson—in which use was not made of the Corollaries introduced by Simson in proving subsequent propositions, Professor Cayley suggested striking out Simson's additions and keeping strictly to the original treatise ; upon which a member of the Senate whispered that perhaps to study it in the original Greek would be better still."

At last, in 1887 and 1888, Oxford and Cambridge both passed regulations that allowed proofs other than Euclid's provided that Euclid's order was not violated. There the matter rested until the present century.

But why did the A.I.G.T. not meet with more tangible success ?

They recognised the need for some study of Geometry before strictly deductive Geometry was begun, but they published nothing to help the preliminary study of Geometry.

Their syllabus and textbook were meant to be more rigorous than Euclid's—one of their cries against Euclid was that it was not completely sound. Had they produced a book or syllabus on the preliminary study of Geometry, would they have been more successful in getting change ? Perhaps, perhaps not ; perhaps the time was not ripe for the big change. But I am convinced that it was the hard work done by the A.I.G.T. that enabled the agitation at the beginning of this century, to cause the fetters of Euclid to be thrown off so quickly.

THE PRESENT CENTURY.

Now I come to the period of which I have personal knowledge. I started teaching at Harrow in 1899; among other work I took the two middle divisions in the Upper School on the Classical side (that would correspond to-day to the part of the school working for the school certificate examination, with all the post-certificate boys, apart from about 20 specialists). I had no special directions about what Algebra and Arithmetic I should do; but I was told that at half-term they would have a paper on Euclid, Book III, the paper consisting entirely of propositions, and that any boy who failed would have to do a paper each Saturday afternoon, until he passed. In the other two terms of the year there were papers on Book I and Book II. The majority of the boys in those divisions professed never to have done a rider in their lives, though most of them could write out propositions fairly well. I had to start with the simplest riders, but riders I made them do. I mention this to show that the learning of Euclid was still dominating the teaching of Geometry.

But let me return to the larger world.

In September 1901 the British Association met at Glasgow and Professor John Perry read a characteristic paper condemning most of the teaching of mathematics in our schools; the British Association also had a Committee considering the teaching of mathematics. In that summer the late Charles Godfrey and I spent a holiday together in the Dolomites and he suggested that we should write a letter to the British Association Committee and persuade other schoolmasters to sign it. We discussed the letter and eventually it was sent to the British Association Committee, and was published in *The Mathematical Gazette*, and became known as the letter of the 23 schoolmasters.

In January 1902 the Mathematical Association appointed its first Teaching Committee. The Committee included several men who had been members of the A.I.G.T. in its later years, notably the late Professor Minchin and Professor Alfred Lodge, whom I am glad to see here to-day. One of these two generally presided at our early meetings. The Committee also included four or five men of under thirty; of these I would particularly mention the late Charles Godfrey and C. O. Tuckey, who is still taking a most active part in the work of the Teaching Committee. In the second and third years of the Committee Professor Forsyth was President of the Association and he frequently presided at our meetings. I remember still Levett's delight that an old pupil of his was the first secretary of the Committee.

At first we met four times in five weeks. We proceeded at once to discuss Geometry, and in one term we had prepared a report on the subject which was published in the *Mathematical Gazette* for May 1902.

We came to the conclusion that it was hopeless to try to abolish Euclid as the Examination Textbook, though many of us wished to

do so; we felt that the diplomatic course was to propose changes that could be made without violating Euclid's order (it is interesting to notice how many of the changes proposed coincided with those I have read to you from the first report of the A.I.G.T. Committee, though that document never came before us); we then hoped to obtain enormous support from the mathematical teachers of the country and almost to compel examining bodies to accept these changes.

About this time the Oxford Local Examinations Delegacy published regulations for their 1903 examinations which said "Any solution which shows an accurate method of geometrical reasoning will be accepted"; and the Civil Service Commission headed one of their papers with "In Geometry the demonstrations and sequence of propositions need not be those of Euclid". But the battle could not be won unless the Universities made some move.

The next landmark in reform was the appointment by Cambridge, in December 1902, of a Syndicate to consider mathematics in the pass examinations of the University. Professor Forsyth was displaying much interest and activity in the reform of the teaching of elementary mathematics. I suppose that it was because I was secretary of the M.A. Teaching Committee that he had me put on that Syndicate. I may say that my senior mathematical master, when he heard that I had been put on, said: "I am very sorry, I wish they had put on someone with greater experience."

The Syndicate meetings were extraordinarily interesting to me: I found myself sitting at the same table with men whom I regarded as the great in the mathematical universe and I felt it was a high honour and I also felt very humble; but I shall never forget the patience with which they listened to me and the kindness shown me by certain members of the Syndicate, particularly by Professors Forsyth and Hobson. Those two were outstanding in their grasp of the difficulties under which the schools laboured.

At the first meeting we considered the Geometry of the Previous Examination. To my surprise, Professor Forsyth said that he would have nothing to do with any report that recommended the retention of Euclid's order; he said that this was an opportunity to get out of bondage. Professor Hobson spoke in the same sense. The Syndicate then appointed a Committee to draw up a schedule of Geometry and this schedule was later adopted with the words: "Any proof of a Proposition shall be accepted which appears to the Examiners to form part of a systematic treatment of the subject"; also "Proofs which are only applicable to commensurable magnitudes shall be accepted." The report of the Syndicate was dated 9th May, 1903.

The great question now was, would the recommendations be passed by the Senate; we feared that flysheets would appear and that every country parson who ever took pupils would come up and vote against the abolition of Euclid. However, a storm was brewing at Cambridge about the report of another Syndicate; and under the

shadow of that storm, owing to Professor Forsyth's careful steering, our report sailed through without a division and Euclid as a textbook ceased to dominate English education. That was in the summer of 1903, less than $1\frac{1}{2}$ years after the Teaching Committee was appointed. The first examination at Cambridge under the new regulations was held in March 1904.

As to what Oxford did in the matter, I have to trust to memory, as I have no papers about it. I am pretty sure that it was either in the Michaelmas term of 1902 or in the Lent term of 1903 that I went to Oxford to confer informally with some members of the Board that controlled Responsions. That Board had the power to alter the regulations for Responsions and, though they did not issue a schedule, they published an announcement giving the same freedom that Cambridge was giving. I think I am right in saying that their announcement was made before the new regulations were actually passed by Cambridge.

Of course the freedom from the bondage of Euclid was abused and misunderstood, and at various times there has been the natural reaction in favour of a fixed order; being free from one set of fetters some people could shackle themselves with another set just for the sake of simplifying examinations. Let me quote what Professor Hobson said about this in his Presidential Address to Section A of the British Association in September 1910.

"There are at the present time some signs of reaction against the recent movement of reform in the teaching of geometry. It is found that the lack of a regular order in the sequence of propositions increases the difficulty of the examiner in appraising the performance of the candidates, and in standardising the results of examinations. That this is true may well be believed, and it was indeed foreseen by many of those who took part in bringing about the dethronement of Euclid as a textbook. From the point of view of the examiner it is without doubt an enormous simplification if all the students have learned the subject in the same order, and have studied the same textbook, but, admitting this fact, ought decisive weight to be allowed to it? I am decidedly of opinion that it ought not. I think the convenience of the examiner, and even precision in the results of examinations, ought unhesitatingly to be sacrificed when they are in conflict—as I believe they are in this case—with the vastly more important interests of education. Of the many evils which our examination system has inflicted upon us, the central one has consisted in forcing our school and university teaching into moulds determined not by the true interests of education, but by the mechanical exigencies of the examination syllabus. The examiner has thus exercised a potent influence in discouraging initiative and individuality of method on the part of the teacher; he has robbed the teacher of that freedom which is essential for any high degree of efficiency." *

* *Nature*, September 1910.

We may safely say that the initial object of the A.I.G.T. was attained after 30 years' work. Ultimately it went through very quickly. Levett was very delighted with our success and not a little envious of the quickness with which it was attained; but it was the work of the A.I.G.T. begun 30 years before that made this possible.

What has happened in the 30 years since acquiring freedom ?

At first there was but little real departure from Euclid: geometrical drawing was introduced, but it was rather aimless and tended to be unrelated to the rest of the Geometry work. The next landmark was the publication by the Board of Education of Circular No. 711, in March 1909; that circular was the first authoritative attempt to give an aim to the new teaching of Geometry. It divided the work into three stages: first a stage in which the pupil was to become familiar with geometrical concepts and to learn some of the language of Geometry; in the second stage the pupil was to become acquainted with some of the more important theorems of Geometry, not by logical proof but by intuition and experiment; then the third stage was the systematising stage, the logical course of deductive Geometry. Excellent suggestions were made for the teaching of each of the three stages. That circular was rewritten and amplified in 1914.

In 1923 the Teaching Committee of the M.A. published a new Geometry report which followed the lines of the Board of Education circular and developed it further. I do not think that report has had all the recognition it deserved; it contained some paragraphs which some people thought "high-brow", and which may have frightened the ordinary teacher. Now the Teaching Committee is actively at work on another Geometry report, not to replace the report of 1923, but to amplify it and bring it up to date. I think we may all expect a good deal of help from the new report when it is published.

Has there been advance in the 30 years since Euclid was dethroned? Looking back over these 30 years, I feel very certain that boys and girls of to-day have much more knowledge of the subject itself and much more power to use what they know. I think that perhaps few of those who taught Euclid remember to-day how small were the fruits of their labours in those days; I have lately studied my mark book of 36 years ago and tried to remember the work we did then and I am convinced that there has been a big advance.

The question may arise some day whether we shall go further and throw overboard from our schools the teaching of systematic Geometry. I shall not live to see that day and I hope that none of you will ever see it. I believe that systematic Geometry taught wisely and with proper introductory work, is a very valuable part of a boy's and a girl's education.

May I consider briefly the progress that has been made in subjects other than Geometry. In Arithmetic we have a better perspective, fewer rules and more understanding. Levett used to tell us that

there are five rules in arithmetic; addition, subtraction, multiplication, division and common sense. At the end of the last century much time was wasted on rules for contracted multiplication and division, rules for converting recurring decimals to vulgar fractions, etc., etc.; also the use of an x in arithmetic was considered a crime. One thing I regret, and that is that I fancy there is less numerical accuracy to-day; we must not lose sight of the necessity for drill in mere computation.

In Algebra I think there has been great advance: the old teaching was largely rule-grinding and heavy manipulation that led nowhere for 95% of the boys who did it. To-day there is much less manipulation and much more understanding of what it all means. Manipulation is easy to test by examination, but why should Algebra teaching be sacrificed to the needs of the examiner? What is the good of giving every boy a manipulative skill that he will never use?

Then too we have graphs which are full of interest and are applied to every sort of subject. It was rather pitiable to see some of the work that was put into some books when graphs were first introduced into examinations and school work: there was a good deal of watered down analytical geometry. But that has all passed away now, I hope, and to-day the graphical work that is done is helpful in making Algebra more full of meaning and is useful in teaching a boy to make deductions from statistics.

Trigonometry is much more widely taught; 35 years ago it was taught to but few boys. The work was largely manipulative, and so far as practical applications were done it was curious that angles of elevation, etc., were almost always either 30°, 45°, or 60°, or, in higher flights, even 15° or 75°. Four-figure tables were almost unknown in the mathematical class-room, though they were used in the Science school. I well remember that at the end of the first term in which we used four-figure tables at Harrow, I was told to set the paper for the few boys on the classical side who did trigonometry; the day before the paper I sent a notice to the boys, telling them to take up their table-books; the senior mathematical master heard of this and said that books were not allowed in examinations. I pointed out that they were essential for the paper I had set, which was strictly on the term's work the boys had done; I was told to set another paper, which I flatly refused to do. After three interviews on the point, the paper which I had set was allowed to stand and table-books were to be used on two conditions: (i) I was to inspect every boy's book to see that he had no helpful notes written in it, (ii) it was to be clearly understood that table-books were never to be used again in an examination. I carefully observed the first condition and duly inspected every boy's table-book. With regard to condition (ii), I may say that table-books have been used in every examination since that date.

To-day Trigonometry is introduced early, and I suppose almost every boy leaving a secondary or public school has some knowledge of the subject.

Perhaps the greatest progress has been made in the earlier introduction to Calculus. It so happened that when I went to Harrow, there was not a single boy in the school who had done any Calculus, though several who had left at the end of the previous term had done a fair amount. I was turned on to teach Calculus to a few boys who wanted it for their work in Science, and with them I tried the experiment of learning to apply the Calculus as soon as a boy could differentiate x^n, $\sin x$ and $\cos x$.

To-day nearly half of our Harrow candidates for School Certificates can use the Calculus so far as differentiating and integrating integral powers of x, and can apply it to finding maxima and minima and areas and volumes. Some schools perhaps do not teach the subject to as many boys as we do, but we have certainly shown that the elementary ideas of the Calculus have an educational value and are not beyond the capacity of more than 40% of our Certificate candidates.

You will notice that I have confined my attention almost entirely to the average pupil. I think I have shown that considerable progress has been made for him and that that progress has been made by cutting down the learning of propositions and rules, and by giving up aiming at the acquisition of mere manipulative power that is not going to be useful to any but the few who are going to specialise in mathematics.

It seems to me to follow that the mathematical specialist, directly after his school certificate examination, must devote considerable time to acquiring manipulative skill. It is interesting to notice how willingly and how quickly this skill is acquired by such boys when they are not hampered by working side by side with other boys to whom such skill is meaningless and useless.

With regard to the specialist whose progress or rather lack of progress was the theme of the Presidential Address last year, I would like to add one word of warning. There is no royal road to learning and we must not expect the standard in scholarship examinations, or for that matter in school certificate examinations, to go on rising indefinitely: the total time given to mathematics at schools does not and should not increase, so that the raising of standard can only be attained by improved methods of teaching and by changes in the subject matter of what we teach, *i.e.* by knocking off non-essentials, and so gaining more time for essentials.

I said that the total time given to mathematics does not and should not increase. Do not let me be misunderstood: I think that up to the Certificate stage every boy and girl should have one mathematical lesson a day; the mathematical specialist will naturally devote the greater part of his time to mathematics, but he or she should devote a proper proportion of time to other subjects, not only to allied subjects like Physics, but also to literary subjects. I should say that to-day in many schools the tendency is for mathematical specialists to devote more time to literary subjects and so less to mathematics, and I believe that is to the good of mathe-

matics ; but I am sorry to hear of some schools in which boys begin to specialise even as early as 14 or 15 and do very little except mathematics—boys from such schools may be very successful in winning scholarships at the University and perhaps in University Examinations, but I do not believe that they will prove as useful in later life either to the Nation or to Mathematics. Too early and too narrow specialisation is dangerous.

I will not say more about the work of the mathematical specialist as we are going to discuss that to-morrow.

One more thing I wish to say : when I became President of your Association I received letters of congratulation, some from people whom I knew and some from people whom I did not know ; one feature was common to many of those letters, let me quote one of them—" Now that we have a schoolmaster president perhaps we shall come down to earth and discuss topics that are of interest to the ordinary schoolmaster."

I should like to say a few words about that. This Association, as I said before, was founded by schoolmasters to improve the teaching of mathematics in our schools. I think I may claim that the reports of our Teaching Committees have done much to attain that end ; but I have heard grumbles that the *Mathematical Gazette* is too " high-brow " and does not perform the function for which it was started. I once wrote to the late editor of the *Gazette* and pleaded for more articles on elementary subjects. In reply I received a characteristic post-card : " Write them ".

I have here a copy of the original notice about the *Gazette* ; it is described as " A journal of Elementary Mathematics, to be devoted entirely to such subjects as are usually taught in secondary schools. This limitation will exclude the Calculus and higher branches, which already possess a current literature of their own." The cover of the first number of the *Gazette* said : " We intend to keep strictly to ' Elementary Mathematics ' : while not absolutely excluding Differential and Integral Calculus, our columns will, as a rule, be devoted to such school subjects as Arithmetic, Algebra, Geometry, Trigonometry, and Mechanics."

Now I propose to defend the *Gazette*. I acknowledge to some extent it has drifted away from its original object ; it has articles and reviews of books on advanced work ; those reviews in particular have given the *Gazette* a reputation that is world-wide, and it is good for all of us to have at any rate a nodding acquaintance with what is being done in the forefront of mathematics—some of our present pupils may be working there in days to come.

But apart from this advanced work we are finding in the *Gazette* an increasing number of articles that are of direct interest and value to the ordinary teacher ; this is partly due to the production of papers read at our branches. We have a sympathetic editor and, without curtailing our excellent reviews, I hope, and I believe he hopes, to see in the *Gazette* more and more articles concerned with elementary work. How can it be done ? By increasing the funds

available for the *Gazette*, and that can only be done by increasing our membership, so do all you can to induce more and more teachers to join the Association.

One more appeal, and that is to the "highbrows"—do be kind to the writer of articles on elementary work. I am sure that the fear of highbrow criticism has, in the past, prevented some excellent teachers from giving to their fellow teachers ideas that would be valuable in the ordinary class-room.

I do not wish to end on too complacent a note: there is still much to be done to improve our mathematical teaching; but I hope that I have convinced you that there has been great progress in the work of all pupils up to the School Certificate stage—I must not go further than that after what was said by our president of last year—and that to this Association is due very much of the progress that has been made.

Last of all I should like to thank you for the patient way in which you have listened to my odd gleanings and reminiscences.

A. W. SIDDONS.

P.S. There are two points that I should like to stress. In the early days of the A.I.G.T. there was a committee of the British Association considering the question of Geometry and the A.I.G.T. was much helped by the committee. Again in the early days of the Teaching Committee there was a British Association committee (of which Professor A. R. Forsyth was chairman and Professor John Perry secretary); the work of this committee was very helpful in securing the abolition of Euclid.

1037. Solution to double acrostic, Gleaning 1030 (*Gazette*, XIX, December 1935), p. 342.

S	eirtemoe	G
I		O
D	emonstrate	D
D	eka	F
O	rthocente	R
N	ine point centr	E
S	ymmetr	Y

(1). "Reflect."

(2). "Hailed" prohibits the insertion of other points. Unfortunately G. & S. themselves do not use O.

(4). "Converse." And we all know that slipshod reasoning is commonest in connection with converses. E. H. N.

1038. Un peuple qui n'accorde pas aux mathématiques un rang élevé dans son estime, ne sera jamais en état de remplir les plus hautes tâches civilisatrices et de jouir, par suite, de la considération internationale qui, elle aussi, constitue à la longue, un moyen efficace de conserver notre situation dans le monde et de sauvegarder notre droit à vivre notre propre vie.—From Mittag-Leffler's will, founding the international institute at Stockholm; *Nature*, July 6, 1926, vol. 97, p. 384. [Per Prof. H. G. Forder.]

OLD TRIPOS DAYS AT CAMBRIDGE, AS SEEN FROM ANOTHER VIEWPOINT.

By Karl Pearson.

Nothing can convince us more that "things are not what they seem" than a comparison of the descriptions by two men of what they remember of Cambridge in the 'seventies! I should of course recognise my friend Professor Forsyth's picture of the Cambridge of that time and am grateful for his calling to my mind so many memories of the past. But for me he has painted them in far too drab a colour, so that one wonders whether his undergraduate days could have been like mine some of the happiest in his life. The reading of his paper enforces upon me the truth of the principle that environment and personalities are not things in themselves, but are our own "constructs"—each of us giving our own "reality" to such evasive phenomena.

Reading Professor Forsyth's paper, one might suppose that, for the mathematical student of the 'seventies, Cambridge life was a dull grind for more than three years under a non-inspiriting "coach", with a vampire examination hanging over us and sucking out our youthful blood—the terrible strain of the so-called Tripos. Well, that may have been the case for some who worked for place and reputation, but surely the vampire conception of the Tripos arose from some of the men who entered for it, rather than from the examination itself? In my personal view the Tripos of those days was an excellent examination, and this for two reasons: First, because it was *not* specialised, but gave a general review of the principia of many branches of mathematical science, and, secondly, because the weight given by it to "problems", enforced "problems" on the teaching coaches. Every bit of mathematical research is really a "problem", or can be thrown into the form of one, and in post-Cambridge days in Heidelberg and Berlin I found this power of problem-solving gave one advantages in research over German students, who had been taught mathematics in theory, but not by "problems". The problem-experience in Cambridge has been of the greatest service to me in life, and I am grateful indeed for it.

Now let me return to those who constructed a vampire out of the Tripos. Like Forsyth I was one of a class of some six or eight freshmen sitting at Routh's feet in 1875, and I wanted to know something personally of these comrades, so I asked one, in our second or third week, to come to breakfast with me. He replied, very politely refusing, because he was too busy working for the Tripos! That is what I have termed the vampire construct of the Tripos examination. It was very nearly the same with the others. They had come up—in several cases after degrees elsewhere—for the Tripos and the Tripos only, not in order to widen their minds from what Cambridge could furnish in all directions, not to leave Cambridge a little better than they found it. The "coaching system" pro-

vided ample time for other lectures and other reading—advice for the latter was always available—and if you did enough mathematical reading for yourself, appreciated your coach's efforts, and were content to be a wrangler, I don't think the vampire element came into your idea of the Tripos. These facts may show that I differ widely in my " construct " from Professor Forsyth's:

> " We were drilled in the gymnastic that led to swift answer according to rule and pattern. In the examination there was no leisure to think : even during our training there had been little leisure for thinking, because we always were being taught ; and independence in reading was almost a misdemeanour in the eyes of some coaches."

Let me question another point in Forsyth's account :

> " With the production of the Tripos list the association of coach and pupil ended. They passed out of one another's lives ; and the coach returned to the same round of drill with the pupils who were to go through the final mill " (p. 178).

In my case at any rate this seems to give a very wrong impression of " coach " and " pupil " ! Let me state my experience. Owing to reasons of health I had been withdrawn from school at sixteen, and spent my seventeenth year with a private tutor who had been Senior Wrangler and a tutor of Trinity. He was not a bad teacher, but unable to manage a party of young men, who did not want to learn and whose follies easily developed into vices. Perhaps the most useful knowledge I acquired from him was an appreciation of Dynamics, and from his pupils an acquaintance with dog-breeding which has been useful to me in after-life. At the end of the year I persuaded my father to let me go up to Cambridge and work as a " beast " under Routh. I knew nothing of Routh, but I called upon him in his quarters so graphically described by Forsyth. He was covered with chalk, had his coat off and his shirt sleeves rolled up, with the sponge and duster in his hand, which were so familiar to his pupils. He was in a great hurry between two lectures, but he looked at me and said, " Come to me at seven o'clock to-morrow morning ", and we parted.

At seven o'clock I went and found another " beast " there, " Josh " Conway (now Lord Conway of Allington). Routh said to us : " You have a year before entering college, we will devote it to reading subjects not of first-class importance for the Tripos ", and he started straight off to lecture on the theory of elasticity. Conway dropped away after a term, and Routh took me alone at seven o'clock in the morning throughout a year, introducing me to Lamé's works and papers by other writers. There was no drill, no attempt to cram. He never referred to the scholarship examination, on which much depended, but left me to work that out for myself. I think Routh had a real affection for me, as I certainly had for him. I was invited several times to his house during my Cambridge career, and never failed afterwards on my rare visits to my Alma Mater to pop in and

shake hands with him at Peterhouse; and years later when my children went up to Cambridge his widow was kindness itself to them. I think others of Routh's pupils could bring evidence to show that he had in them an interest which extended beyond the Tripos list.

Nor can I agree with Professor Forsyth when he says:

> "The imaginary *i* was suspiciously regarded as an untrustworthy intruder. The complex variable (a phrase that had not then penetrated to Cambridge) was described either as imaginary or impossible" (p. 172).

On the contrary, I heard of the "complex variable" both from Burnside and Frost as well as from Routh himself, who used to give a clear if an elementary account of it. Beyond this, some of us, who in those years read the *Messenger* and *Quarterly Journal of Mathematics*, were confronted with its physical applications and did due honour to its possibilities. I cannot say that anybody directly advised me, but some one or other of my teachers led me, well before my Tripos, to other works of Salmon than his conic sections; there was $T+T'$ (not T and T' be it noted!), the first volume of Lord Rayleigh's *Sound*, Green's Papers, Clerk Maxwell's great volumes, De St. Venant's edition of Clebsch and his own papers, there was a book on modern geometry by Townsend, Ball on *Screws* (which was undoubtedly recommended by Routh himself), and many more, if I were in my library and could examine the dates of purchase. Besides Lamé's four works and papers by Fresnel, there was Fourier's treatise and undoubtedly other French books. In German, Kirchhoff's *Mechanik*, Heine and Neumann on spherical harmonics and Bessel's functions, Durége's *Elliptische Functionen*, and a work by Riemann whose title escapes my memory. I do not refer to a list which might be considerably lengthened to show that undergraduate reading extended beyond coach's lectures, but to prove that Cambridge teachers three years before Forsyth went up led their students to read foreign literature. I started at Cambridge with no acquaintance with the standard works of mathematics, and my knowledge at least of their titles must have come from my teachers, Routh, Burnside and Frost. Hence I cannot again agree with Forsyth when he writes:

> "But all such ventures were exceptional and rare: there was no advice, no wish, no leisure, to urge us on those paths. The coach was the autocratic director, often the sole director" (p. 175).

I contend that there was ample advice, ample desire, and, above all, ample leisure for those who put aside that notion of the Tripos place being their one object for three years of their life! Talking about German mathematical works, the door to their study was opened to me by a delightful old German, Steinhelfer, who read Heine's *Reisebilder* with me in my first term. His death soon after left his widow penniless, and this brought me into slight contact

with a big personality, Henry Jackson of Trinity. Another great figure of that time was Munro. I only once came in contact with him. There was in those days a curious habit in Trinity of examining undergraduates of all years for foundation scholarships on the same papers as those seeking entrance scholarships. Conway and I sat for the examination, but we had to sit for both mathematical and classical papers. One of the latter was a Latin composition paper set by Munro, who was walking up and down the hall. He saw me idle, and he came up and entreated me with almost the tenderness of a father to write a few Latin verses! I would have done anything to comply with his request and please him, but I had only once as a schoolboy been set to make verses and then—I got my uncle to make them for me. That was one experience of my Trinity examination. Another was this, there was a mathematical paper, of which about a third was occupied with questions as to certain mysterious $P_n(x)$'s and $Q_n(x)$'s. These were defined by differential expressions, and the examinee was asked to prove numerous propositions with regard to them. I was much struck by the ingenuity of the examiner who could invent such interesting functions and deduce such remarkable properties of them. The questions fascinated me and I devoted my chief attention to them. Some week or more after I had a letter from Glaisher stating that the examiners rejected me, considering that I had devoted too much time to the higher branches of mathematics, neglecting the fundamental subjects. I thought the examiners must have heard of my reading Lamé's works with Routh; only some time later did I discover that I had been answering bookwork questions on Legendre's functions of which I had never heard previously. I have since held that there were worse examinations in Cambridge in 1875 than the Mathematical Tripos!

I had probably nothing like the same acquaintance with the mathematical leaders in Cambridge of the 'seventies as Professor Forsyth had, for I left Cambridge for Germany shortly after my Tripos and did not return permanently to it. But some little account of my contact with one or two of them may be of interest. Our college lecturer in mathematics was a dear old boy, Percival Frost, but he had to lecture down to the lowest individual in the class, and accordingly his lectures were of no value to Routh's pupils. But the college law was inexorable; every undergraduate *must* go to college lectures. At last a compromise was arranged: I should go to Frost's private house twice a week for an hour. He had a large study; on one side were a good collection of standard mathematical works, on the other at any rate an equally voluminous set of theological writers. "Good morning, Mr. Pearson, what difficulties shall we discuss to-day?" "Good morning, Mr. Frost, can you explain to me this sentence in Tillotson's sermons?" Then followed a theological discussion for at least half an hour. Frost, with a twinkle in his eye: "You are as aggravating as Kingdon Clifford; aye, but he was a mathematician! The best pupil I ever

had. But last Wednesday we were talking about geodesics; it is time to get back to them." Perhaps we did, but oftener we didn't. Spinning billiard balls round silk hats, and the theory of doing so, was his delight, and in vacation time he might be induced to enter the billiard room of an inn and display his prowess. His solid geometry and his wonderful book on curve tracing were excellent treatises in my day. His *Newton* only gave one an appetite which failed to be satisfied in my time at Cambridge. He was also a good hand at backgammon. Once I asked him why he always beat me. He replied: "I always calculate the probability before I make a move. I don't think you do." One day he said to me: "Mr. Pearson, I have a nice mathematical problem for you. When I have shaved, I drop the shaving paper into a certain article, but it never will fall in. Can you account for it?" That was my first problem in aeronautics, and, of course, I failed to solve it; and then the mild twinkle came into his eye, and from his mouth: "Aye, but Clifford would have solved it; he *was* a mathematician." I cannot bring Frost under Professor Forsyth's description of the "autocratic coach".

With Adams my contact was small, but perhaps worth recording. I think in my second term an Italian, Signor Nathan, came to lecture on Dante in Cambridge. There were then no Italian teachers, no *Modern Languages Tripos*. On the first day there was a fair audience, but after the third lecture only Adams and I were left to keep Nathan in countenance. It was probably unnoticed by Adams; he would be used to small audiences. My next meeting with Adams was of a more painful kind. I held the respect for him current in our time at Cambridge. One day in vacation time I was playing tennis in the grounds of an hotel at St. Ives, Cornwall, when up the drive came an old-fashioned pair followed by a porter carrying one large skin-covered trunk on his back—it was all a little out of date even fifty-eight years ago. But I was thrilled; here *was* a chance! I returned to my game, but I had hardly served when I saw the back of the porter, the hairy trunk and the old-fashioned pair, obviously tired, retreating down the drive! I threw down my racquet, for I knew there were vacant rooms in the hotel, and rushed to the manageress. "What have you done?" I cried; "you have turned away the discoverer of Neptune." "Neptune or no", she replied, "I am not going to have dowdies like those in this hotel!" Such is the fate of genius if it does not put on its best clothes when it enters a big hotel.

Professor Forsyth tells us little about the Smith's Prize Examination, which was in those days very characteristic of the best in Cambridge mathematics. Four professors examined, in my year Stokes, Clerk Maxwell, Cayley and Todhunter, in place of Challis, the Professor of Astronomy, who was ill. We went on each occasion to the examiner's house, did a morning paper, had lunch there, and continued our work on the paper in the afternoon. At Stokes' we had a family lunch, the professor at one end of the table and his

wife at the other, also a daughter and I think a son were present—I may now admit a silent admiration for that very beautiful girl, she frequently came to King's College Chapel Services, and had considerable attraction for the scholars in the stalls. Professor Forsyth may be interested to know that one of the questions put by Stokes was, " Write an essay on the theory of functions ", or words to that effect. For Stokes I held a great veneration. His wonderful look of satisfaction when an experiment turned out successfully, and it generally did, is not really describable ; it was as if he had made for the first time a most important discovery. A few only of Routh's pupils of my year went, and none but I attended the continuation of his lectures into a second term. " It did not pay for the Tripos." I have only met one lecturer as good as the Stokes of those days, namely, Quincke of Heidelberg. He planned his course so as to fit his topic to his time, and never finished his hour and his lecture in the middle of an experiment. The first words almost he said to me were : " From Cambridge ? Stokes is there ; the greatest mathematical physicist in England." Years later I was standing by Quincke at the Stokes Jubilee when Lord Kelvin made the remarkable statement that Stokes' room in Pembroke was the first physical laboratory in Europe. Quincke murmured, " Franz Neumann ", and I could only nod assent.

At Clerk Maxwell's we did our papers in the dining-room and adjourned for lunch to an upper room, probably the drawing-room, where Clerk Maxwell himself presided. The conversation turned on Darwinian evolution ; I can't say now how it came about, but I spoke disrespectfully of Noah's Flood. Clerk Maxwell was instantly aroused to the highest pitch of anger, reproving me for want of faith in the Bible ! I had no idea at the time that he had retained the rigid faith of his childhood, and was, if possible, a firmer believer than Gladstone in the accuracy of Genesis. His books, from the little treatise on heat to the revelations contained in his two volumes on electricity and magnetism, were splendid ; but he could not lecture. He seemed to have no planned course ; and after three weeks of his course on heat, I beat a retreat. It was not solely because " the lectures were no good for the Tripos ", but partly because I grew weary of seeing his demonstrator standing, with his thumbs in his waistcoat pockets, almost behind the professor as the latter struggled to boil water. However, his Smith's Prize paper was a treat which we could all enjoy.

The next day we went to Cayley's. His first words were, " Throw off your gowns, gentlemen, you will work more easily without them ", and accordingly they were dropped in a heap in a corner of the room, and we set to work unencumbered. Of course I knew nothing of the topics of Cayley's paper. My chance of scoring marks in the Tripos had depended only on my applied mathematics, and my pure mathematics were but sufficient to help in the former branch. But I took things leisurely, as if nothing depended on speed, and worked as one might work in solving crossword puzzles on a train journey.

Cayley did not appear at lunch ; sandwiches, biscuits and other light refreshments were brought up on a tray, accompanied by a decanter of excellent port wine ; Cayley had not spared his cellar. After sampling a glass, I tried to persuade my co-examinees to do so likewise ; two, I think, took a driblet, but the future Smith Prizeman, speaking from his conscience, refused—he was true to what he had originally said in our first term. He had come to Cambridge for examination ends ; perhaps he thought I was tempting him to drop the prize already well within his grasp. Back we went to our writing, I feeling the better for Cayley's port, and the others satisfied in their consciences that they had done the right thing under examination stress. Cayley evidently did not think good port at all incompatible with the discussion of invariants or higher algebra. A few days later a friend of Cayley's told me that Cayley had remarked that there was only one man who appeared to have thoroughly enjoyed his paper—it was the one man who thoroughly enjoyed his port. Somehow that commendation was more to me than if I had won a Smith's Prize or gratified Routh or my college in being senior.

The last day we went to Todhunter's. I do not think any of us knew more of him than what we had gathered from his textbooks, or, perhaps in a few cases, from his two histories. He held no teaching position. What we did not know was that he was a strict disciplinarian. He came into the room with his papers in his hand. He stood aghast, the papers fell from his hand—mindful of the greater Cayley's permission, we had dropt our gowns in the corner of the room ! "Put on your gowns, gentlemen, at once ; this is an unheard-of irregularity." Crestfallen, we resumed our academic costume, but with us Cayley was reckoned still higher in the scale of Cambridge worthies than ever before ! But Todhunter's paper strangely enough made a turning-point in my career. There was a question in it on either the torsion or flexure of prisms which I answered, having read De St. Venant's memoirs. I thought I had answered it as the author himself had done. But it was not so. Some few years afterwards I was asked by the Syndics of the Press to finish and edit Todhunter's *History of Elasticity*. I accepted, but had not the least idea of how it came about. When I got the manuscript, however, I found a considerable number of pages of my Smith Prize paper had been incorporated into it by Todhunter, with the pencil remark that the proof was better than De St. Venant's. It had evidently been seen by the referees ! Thus from that day with its "unheard-of irregularity" started my link with Todhunter, and to me a more vital association—that with the University Press, whose familiar proofs have for some fifty-five years scarcely ever been missing from my writing table !

I have endeavoured to indicate how in the Old Tripos Days at Cambridge it was possible to enjoy thoroughly university life without thinking of the Tripos as a vampire limiting all one's freedom. It was not the fault of the examination that some men became

slaves to it, played no games, read nothing but examination mathematics, came in contact neither with the minds of the better teachers, nor widened their own minds by general reading or interchange of thought with their younger contemporaries in other branches of study. Has the abolition of the Senior Wranglership and the division of the Tripos into specialised sections improved matters ? I very much doubt it. I know that, when I had to seek for assistants from Cambridge, I often came across my old friend, who had spent his time there working solely for examination, and, further, I found that owing to his specialisation he could rarely lecture on two heads of multicipital mathematics. It was, and I still think is, the fault of the colleges, who look only to marks in examination papers. Even if they do elect a certain number of such single-idea'd scholars, they ought to humanise them, drive out of them the commercial view of the university career as a step to obtaining a livelihood, and force them to take part in the general college and university life. It is needless to say that I am not looking at Professor Forsyth as having been an undergraduate of that type ; his account of his own mathematical reading shows that he was not. But I want to protest against his view of the Old Tripos. If it played the vampire to some men, it was partly their fault and partly the fault of the college tutors.

Professor Forsyth may say that I was singularly fortunate. It is true that I belonged to a small community, some thirty-two members in all, of whom a moiety at least were Etonians. So small was our number that the rules of the press-gang had to be applied to the dons in order to man the boat. But our very smallness brought us in far closer community with the dons than was possibly customary in other colleges, and several of them helped much to humanise us cubs. There was Oscar Browning—very good for freshmen in their first or even second terms—he introduced me to Rousseau, Goethe and Italy. There was G. W. Prothero, a man who could hold the balance between the old and the new, between the mediaeval provost and the hustling undergraduate. But the man who most influenced our generation was Henry Bradshaw—best for a man in his third year. Those who had made his friendship would soon lose all conception of a vampire Tripos examination !

Of course, there were other dons of the old school who could not accept the transition that the college was undergoing, and they had to be fought, which was an additional pleasure in life. There were then compulsory divinity lectures dating back to the foundation of the college. After sitting through lectures on the Thirty-Nine Articles and the Prayer Book from a lecturer who did not throw as much life and light even into the latter as the *Interleafed Prayer Book* does, I asked the tutor if I might go to Westcott's lectures instead. After some demur he permitted me, and I sat among the divinity students for a term listening to a most learned but scholastic discourse on whether Christ would have come to earth if Adam had not eaten the apple. Westcott concluded that he would ; it was

needful for the perfection of the Godhead. I felt inclined to ask him, "For whose sake?", but feared he would think it ribald. However, that was enough; I went to the tutor and said, "I am not going to attend any more divinity lectures". He looked aghast. "It is an inexorable rule of the college." "Then I am going to another college". "It is unheard of. We shall not give you a *bene decessit*". "Mr. Ferrers of Caius has already agreed to accept me without a *bene decessit*." "This matter must come before the College Council." It did come, and after a hard fight compulsory divinity lectures were abolished. The matter was not ended. The tutor came to my rooms—like Nicodemus, by night—"You have had your way, but it will break the discipline of the college for a scholar to do this. I should like you to sit for an examination in Hooker's *Ecclesiastical Polity*." "That is to hang a man, after capital punishment has been abolished." However, I agreed smilingly to read the judicious Hooker's book if I were not examined on it; it might be interesting. I kept my promise, but I don't think I drew so much amusement and profit from Hooker as from Paley. I only once later came in touch with Westcott. I have said that our community was a small one, and I sympathised a good deal with a man who was the very first student of the "Moral Sciences" in the college. As a freshman, he was rather in trouble; the college tutor had told him that while he had been compelled to get up the conditions of the Mathematical Tripos, he could not be expected to do so for the Moral Sciences Tripos. As a matter of fact, till the irruption of non-Etonians, he had never had to busy himself with anything but the Classical Tripos. Well, the student of the Moral Sciences and I fraternised, especially over Spinoza, and before I finally broke the link with Cambridge I had written in the *Cambridge Review* notices of Pollock's and of Martineau's books on Spinoza, as well as a paper on Maimonides and Spinoza in *Mind*. At a college meeting I again met Westcott. He said to me sorrowfully: "Ah! when I was a young man, I also admired Spinoza." A reply was on my lips, but I refrained. I can only say that till this day I think Spinoza the sole philosopher who provides a conception of the Deity in the least compatible with scientific knowledge.

Of a certainty there were many occasions for action, mostly of a profoundly interesting kind in Cambridge in the Old Tripos Days, even for the mathematician. Compulsory divinity lectures had disappeared, it was time for compulsory chapels to go likewise. I went to the tutor and told him that I objected to compulsory chapels. "Are you a dissenter?" he asked. I could not say that I was, but I appealed to the Test Act. "Well, you are not yet nineteen, and that is a matter for your father to settle." I was sorry to drag my father into the affair, but if one could not settle one's own religion at nineteen years of age, one had to do so. He signed the document of protest much as he would have signed one of his legal opinions or a cheque. He was not a good churchman; he

spent most of his Sundays in what he termed " helping the ass out of the ditch ". Armed with the requisite document, I was freed from chapel attendance. On the following Sunday, to the astonishment of the tutor and deans, I appeared in my usual stall in the chapel. The next morning I was summoned to the dean's rooms: " You demanded to be released from attendance at chapel, and you were there yesterday ! " " No, Sir, I asked to be released from *compulsory* attendance at chapel, and I hope to be there when the spirit moves me." Thus I went when I liked after that; nothing further was said to me. But the situation was impossible. Soon after that compulsory chapels were abolished, and I felt I had done my bit towards modernising that great college in my undergraduate days. No doubt some will say that what I attempted was not true progress. That is not my point; I do not wish to pose as a reformer. I want to point out that in the Old Tripos Days at Cambridge it was possible for an individual thoroughly to enjoy his undergraduate career, to read mathematics outside the Tripos range, and a good many other subjects as well, to find " coaches " who led him on and grew to be friends, to find college authorities who on the whole had some sense of humour and were not wholly upset by his want of respect for the " conventions ", and lastly, and best of all, to have no vampire Tripos conception hanging over him for more than three years. There was pleasure in the friendships, there was pleasure in the fights, there was pleasure in the coaches' teaching, there was pleasure in searching for new lights as well in mathematics as in philosophy and religion. I think I carried away from Cambridge my full share of the benefits it can bring to any of its sons. Possibly, if I had stayed in Cambridge, the memory of my undergraduate time would have lost some of its sunlight. My " construct " now of " Old Tripos Days at Cambridge " differs widely from Professor Forsyth's. Is it the different natures of the constructors which makes their description of what they experienced so different ? Or is it that friend Forsyth is really defending the reforms—for which he was so largely responsible—and which destroyed for ever the venerable Cambridge Mathematical Tripos with its value as a general education ? To-day the Cambridge Tripos produces mathematical specialists, but hardly as many men of distinction in all branches of State service.

K. P.

The Old Schoolhouse, Coldharbour,
September 20th, 1935.

1039. De Philosophis.

Hic Diogenes olim patria expulsus erat quia nummos adulterinos fecerat; itaque malus philosophus erat. Nam boni philosophi (ut supra demonstravimus — omnes boni scriptores supra demonstrant) nunquam utile quicquam faciunt.—G. M. Lyne, *Balbus*, p. 17. [Per Mr. V. Naylor.]

SOME GEOMETRICAL APPLICATIONS OF VECTORS.

By H. Lob.

The aim of the following notes is merely to indicate the straightforward way in which vectors lend themselves to geometry. Some of the work is less direct than it might be, but this is partly due to the desire to keep to first principles as much as possible.

The chain of theorems in § 6 has been suggested by a chain constructed many years ago by Mr. H. W. Richmond. In this, points are taken on a unit circle and the second unit circle through each pair of points is drawn; the centres give rise to a chain of unit circles.

1.1. *Relation between mutual distances of* 4 *points in a plane.*

Let O, A, B, C be the four given points.

Take $\overline{OA}=\alpha$, $\overline{OB}=\beta$, $\overline{OC}=\gamma$.

Then, since the vectors are in one plane,

$$\gamma = l\alpha + m\beta,$$

where l, m are numbers.

Taking scalar products of γ with respect to α, β, γ in turn, we have

$$\left.\begin{aligned} l\alpha^2 + m\alpha\,.\,\beta &= \gamma\,.\,\alpha \\ l\alpha\,.\,\beta + m\beta^2 &= \beta\,.\,\gamma \\ l\alpha\,.\,\gamma + m\beta\,.\,\gamma &= \gamma^2 \end{aligned}\right\}.$$

Thus

$$\begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ \beta\,.\,\alpha & \beta^2 & \beta\,.\,\gamma \\ \gamma\,.\,\alpha & \gamma\,.\,\beta & \gamma^2 \end{vmatrix} = 0.$$

The scalar products can all be put in terms of the mutual distances of O, A, B, C.

E.g. if $OA=a$, $OB=b$, $OC=c$, $BC=p$, $CA=q$, $AB=r$, we have

$$\begin{aligned} 2\beta\,.\,\gamma &= 2bc\cos B\hat{O}C \\ &= b^2 + c^2 - p^2. \end{aligned}$$

1.2. *Relation between mutual distances of* 5 *points in space.*

Let O, A, B, C, D be the points.

Take $\overline{OA}=\alpha, \ldots \overline{OD}=\delta$.

Then $\quad \delta = l\alpha + m\beta + n\gamma$.

Hence

$$\left.\begin{aligned} l\alpha^2 + m\alpha\,.\,\beta + n\alpha\,.\,\gamma &= \alpha\,.\,\delta \\ l\alpha\,.\,\beta + m\beta^2 + n\beta\,.\,\gamma &= \beta\,.\,\delta \\ l\alpha\,.\,\gamma + m\beta\,.\,\gamma + n\gamma^2 &= \gamma\,.\,\delta \\ l\alpha\,.\,\delta + m\beta\,.\,\delta + n\gamma\,.\,\delta &= \delta^2 \end{aligned}\right\}.$$

Thus

$$\begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma & \alpha\,.\,\delta \\ \alpha\,.\,\beta & \beta^2 & \beta\,.\,\gamma & \beta\,.\,\delta \\ \alpha\,.\,\gamma & \beta\,.\,\gamma & \gamma^2 & \gamma\,.\,\delta \\ \alpha\,.\,\delta & \beta\,.\,\delta & \gamma\,.\,\delta & \delta^2 \end{vmatrix} = 0,$$

and as before $$2\beta \,.\, \gamma = b^2 + c^2 - p^2,$$
and so for the other scalar products.

The method obviously extends to space of n dimensions.

2. *Circumsphere of a tetrahedron.*

Let $\rho = x\alpha + y\beta + z\gamma$ be the vector to the circumcentre.

Then $$\rho^2 = (\rho - \alpha)^2 = (\rho - \beta)^2 = (\rho - \gamma)^2,$$
so that $$\left.\begin{aligned} 2\rho \,.\, \alpha &= \alpha^2 \\ 2\rho \,.\, \beta &= \beta^2 \\ 2\rho \,.\, \gamma &= \gamma^2 \end{aligned}\right\}.$$

Hence $$\left.\begin{aligned} x\alpha^2 + y\alpha \,.\, \beta + z\alpha \,.\, \gamma &= \frac{\alpha^2}{2} \\ x\alpha \,.\, \beta + y\beta^2 \quad + z\beta \,.\, \gamma &= \frac{\beta^2}{2} \\ x\alpha \,.\, \gamma + y\beta \,.\, \gamma + z\gamma^2 \quad &= \frac{\gamma^2}{2} \end{aligned}\right\}.$$

Also $$\begin{aligned} \rho^2 &= \rho \,.\, \rho \\ &= x\rho \,.\, \alpha + y\rho \,.\, \beta + z\rho \,.\, \gamma. \end{aligned}$$

Thus $$2\rho^2 = x\alpha^2 + y\beta^2 + z\gamma^2.$$

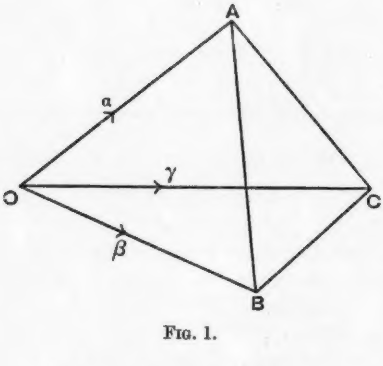


Fig. 1.

Hence $$\begin{vmatrix} \alpha^2 & \beta^2 & \gamma^2 & 2\rho^2 \\ \alpha^2 & \alpha \,.\, \beta & \alpha \,.\, \gamma & \dfrac{\alpha^2}{2} \\ \alpha \,.\, \beta & \beta^2 & \beta \,.\, \gamma & \dfrac{\beta^2}{2} \\ \alpha \,.\, \gamma & \beta \,.\, \gamma & \gamma^2 & \dfrac{\gamma^2}{2} \end{vmatrix} = 0,$$

i.e.

$$\begin{vmatrix} \alpha^2 & \beta^2 & \gamma^2 & 4\rho^2 \\ \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma & \alpha^2 \\ \alpha\,.\,\beta & \beta^2 & \beta\,.\,\gamma & \beta^2 \\ \alpha\,.\,\gamma & \beta\,.\,\gamma & \gamma^2 & \gamma^2 \end{vmatrix} = 0.$$

Hence $4\rho^2 \begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ \alpha\,.\,\beta & \beta^2 & \beta\,.\,\gamma \\ \alpha\,.\,\gamma & \beta\,.\,\gamma & \gamma^2 \end{vmatrix} = - \begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma & \alpha^2 \\ \alpha\,.\,\beta & \beta^2 & \beta\,.\,\gamma & \beta^2 \\ \alpha\,.\,\gamma & \beta\,.\,\gamma & \gamma^2 & \gamma^2 \\ \alpha^2 & \beta^2 & \gamma^2 & 0 \end{vmatrix},$

two symmetrical determinants.

Obviously this can also be extended to space of n dimensions.

3. *Volume of a tetrahedron.*

Let AN be the perpendicular from A to face OBC, and let $\overline{NA} = \varpi$.
Then $\alpha - \varpi$ is in plane OBC, and is thus of form $l\beta + m\gamma$.

Hence
$$\left.\begin{aligned} \alpha^2 - \alpha\,.\,\varpi &= l\alpha\,.\,\beta + m\alpha\,.\,\gamma \\ \alpha\,.\,\beta &= l\beta^2 + m\beta\,.\,\gamma \\ \alpha\,.\,\gamma &= l\beta\,.\,\gamma + m\gamma^2 \end{aligned}\right\} \quad \begin{aligned} & \\ &[\text{since } \varpi\,.\,\beta = 0] \\ &[\text{since } \varpi\,.\,\gamma = 0] \end{aligned}$$

Thus
$$\begin{vmatrix} \alpha^2 - \alpha\,.\,\varpi & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ \beta\,.\,\alpha & \beta^2 & \beta\,.\,\gamma \\ \gamma\,.\,\alpha & \gamma\,.\,\beta & \gamma^2 \end{vmatrix} = 0.$$

Hence
$$\begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ \beta\,.\,\alpha & \beta^2 & \beta\,.\,\gamma \\ \gamma\,.\,\alpha & \gamma\,.\,\beta & \gamma^2 \end{vmatrix} = \begin{vmatrix} \alpha\,.\,\varpi & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ 0 & \beta^2 & \beta\,.\,\gamma \\ 0 & \beta\,.\,\gamma & \gamma^2 \end{vmatrix}$$
$$= \alpha\,.\,\varpi\,[\beta^2\gamma^2 - (\beta\,.\,\gamma)^2].$$

But
$$\begin{aligned} \beta^2\gamma^2 - (\beta\,.\,\gamma)^2 &= b^2c^2 - b^2c^2 \cos^2 B\hat{O}C \\ &= 4 \times \text{square of } \triangle BOC, \end{aligned}$$

and
$$\begin{aligned} \alpha\,.\,\varpi &= OA\,.\,NA \cos O\hat{A}N \\ &= NA^2. \end{aligned}$$

Hence
$$\begin{vmatrix} \alpha^2 & \alpha\,.\,\beta & \alpha\,.\,\gamma \\ \beta\,.\,\alpha & \beta^2 & \beta\,.\,\gamma \\ \gamma\,.\,\alpha & \gamma\,.\,\beta & \gamma^2 \end{vmatrix} = 36V^2,$$

where V is the volume of the tetrahedron. (*Cf.* § 1·1.)

4. *Equation of sphere circumscribing the tetrahedron.*

If $(xyzt)$ are the tetrahedral coordinates of a point P,
then
$$\overline{OP} = x\alpha + y\beta + z\gamma.$$

For P is the centroid of particles of mass x at A, y at B, z at C, t at O, and so

$$\begin{aligned} x\alpha + y\beta + z\gamma &= (x + y + z + t)\,\overline{OP} \\ &= \overline{OP}. \end{aligned}$$

Let now P be a point on the circumsphere.

Then, as in § 2, if ρ is the vector to the circumcentre,

$$2\rho \,.\, \overline{OP} = \overline{OP}^2.$$

Thus $$2\rho \,.\, (x\alpha + y\beta + z\gamma) = (x\alpha + y\beta + z\gamma)^2,$$

i.e. $$x\alpha^2 + y\beta^2 + z\gamma^2 = (x\alpha + y\beta + z\gamma)^2. \quad \text{.....................(i)}$$

If expanded, this becomes

$$xa^2 + yb^2 + zc^2 = x^2a^2 + y^2b^2 + z^2c^2 + \Sigma yz\,(b^2 + c^2 - p^2).$$

And of course we may make the equation homogeneous by multiplying the left-hand side by $x + y + z + t$.

In $[n]$ the corresponding formula is

$$(x_1\alpha_1 + x_2\alpha_2 + \ldots + x_n\alpha_n)^2 = x_1\alpha_1{}^2 + x_2\alpha_2{}^2 + \ldots + x_n\alpha_n{}^2.$$

5.1. *Vector products in connection with tetrahedron.*

Let AN be perpendicular from A to opposite face, and let $NA = h_1$ and vector $\overline{NA} = \varpi_1$.

Then we have the vector product $\beta \times \gamma$ equal to a vector parallel to ϖ_1 and of magnitude $bc \sin B\hat{O}C$.

Hence $$\beta \times \gamma = \text{a vector of magnitude } \frac{6V}{h_1},$$

where $$V = \text{volume of tetrahedron.}$$

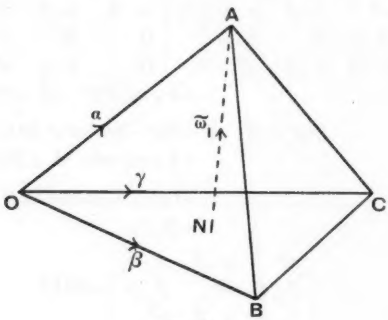


FIG. 2.

$$\left.\begin{aligned} \text{Thus}\qquad \beta \times \gamma &= \frac{6V}{h_1{}^2}\varpi_1. \\ \text{Similarly,}\qquad \gamma \times \alpha &= \frac{6V}{h_2{}^2}\varpi_2, \\ \alpha \times \beta &= \frac{6V}{h_3{}^2}\varpi_3, \\ \overline{AC} \times \overline{AB} &= \frac{6V}{h_4{}^2}\varpi_4 \end{aligned}\right\} \quad \text{.......................................(i)}$$

Now $$\overline{AC}\times\overline{AB}=(\gamma-\alpha)\times(\beta-\alpha)$$
$$=\gamma\times\beta-\gamma\times\alpha-\alpha\times\beta,\quad \text{since } \alpha\times\alpha=0,$$
$$=-\frac{6V}{h_1^2}\varpi_1-\frac{6V}{h_2^2}\varpi_2-\frac{6V}{h_3^2}\varpi_3.$$

Hence $$\frac{\varpi_1}{h_1^2}+\frac{\varpi_2}{h_2^2}+\frac{\varpi_3}{h_3^2}+\frac{\varpi_4}{h_4^2}=0. \quad \text{.........................(ii)}$$

This is equivalent to the property that forces perpendicular to the faces of a tetrahedron and of magnitudes proportional to the areas of the faces, all acting inwards, have no resultant force.

By taking the scalar product of the left-hand side with any vector σ we see that it shows also that the projections of the faces on a plane perpendicular to σ are zero.

Again, $$(\beta\times\gamma)\,.\,\alpha=\frac{6V}{h_1^2}\varpi_1\,.\,\alpha$$
$$=6V,\quad \text{since } \varpi_1\,.\,\alpha=h_1^2,$$
and similarly, $$(\alpha\times\beta)\,.\,\gamma=(\gamma\times\alpha)\,.\,\beta=6V.$$

5.2. *Shortest distance between opposite edges of tetrahedron.*

Let PQ be the shortest distance between OA and BC, and let $\overline{PQ}=\rho$.

Then, since OA and BC are perpendicular to ρ,
$$\alpha\,.\,\rho=0 \quad\text{and}\quad (\beta-\gamma)\,.\,\rho=0.$$

Also, since the projection of OB on PQ is $-PQ$, we have
$$\beta\,.\,\rho=-\rho^2=\gamma\,.\,\rho.$$

It is easily verified that $\rho=-\rho^2\left(\dfrac{\varpi_2}{h_2^2}+\dfrac{\varpi_3}{h_3^2}\right)$.

Hence $$\rho^2=\rho^4\left(\frac{\varpi_2}{h_2^2}+\frac{\varpi_3}{h_3^2}\right)^2$$
$$=\rho^4\left(\frac{1}{h_2^2}+\frac{1}{h_3^2}+2\frac{\varpi_2\,.\,\varpi_3}{h_2^2h_3^2}\right),\quad \text{since } \varpi_2^2=h_2^2 \text{ and } \varpi_3^2=h_3^2.$$

Thus $$\frac{1}{PQ^2}=\frac{1}{h_2^2}+\frac{1}{h_3^2}+2\frac{\varpi_2\,.\,\varpi_3}{h_2^2h_3^2} \quad \text{.........................(iii)}$$

Taking the three shortest distances, we have
$$\sum\frac{1}{PQ^2}=2\left(\frac{1}{h_1^2}+\frac{1}{h_2^2}+\frac{1}{h_3^2}\right)+2\left(\frac{\varpi_2\,.\,\varpi_3}{h_2^2h_3^2}+\frac{\varpi_3\,.\,\varpi_1}{h_3^2h_1^2}+\frac{\varpi_1\,.\,\varpi_2}{h_1^2h_2^2}\right)$$
$$=\frac{1}{h_1^2}+\frac{1}{h_2^2}+\frac{1}{h_3^2}+\left(\frac{\varpi_1}{h_1^2}+\frac{\varpi_2}{h_2^2}+\frac{\varpi_3}{h_3^2}\right)^2,\quad \text{since } \frac{\varpi_1^2}{h_1^4}=\frac{1}{h_1^2},\ldots$$
$$=\frac{1}{h_1^2}+\frac{1}{h_2^2}+\frac{1}{h_3^2}+\frac{1}{h_4^2},\quad \text{from equation (ii) of §5.1.}$$

5.3. Since
$$-\frac{\varpi_4}{h_4^2}=\frac{\varpi_1}{h_1^2}+\frac{\varpi_2}{h_2^2}+\frac{\varpi_3}{h_3^2},$$
we have, by squaring,
$$\frac{1}{h_4^2}=\frac{1}{h_1^2}+\frac{1}{h_2^2}+\frac{1}{h_3^2}+2\sum_1^3\frac{\varpi_2\cdot\varpi_3}{h_2^2h_3^2},$$
which is equivalent to
$$(\triangle ABC)^2=(\triangle OBC)^2+(\triangle OCA)^2+(\triangle OAB)^2$$
$$-2\Sigma(\triangle OBC)(\triangle OCA)\cos(OBC,\widehat{}\,OCA),$$
a curious extension of Euclid, II. 12, 13.

If OA, OB, OC are mutually perpendicular, we get
$$(\triangle ABC)^2=(\triangle OBC)^2+(\triangle OCA)^2+(\triangle OAB)^2$$
and
$$\frac{1}{h_4^2}=\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}.$$

6.1. *The sphere through the centroids of the faces.*

Changing the notation, we take A_1, A_2, A_3, A_4 as the vertices of the tetrahedron and the origin, O, at the circumcentre.

Then if
$$\overline{OA_1}=\alpha_1,\ \overline{OA_2}=\alpha_2,\ \dots,$$
the centroids G_1, G_2, ... are given by
$$\overline{OG_1}=\tfrac{1}{3}(\alpha_2+\alpha_3+\alpha_4),\quad \overline{OG_2}=\tfrac{1}{3}(\alpha_1+\alpha_3+\alpha_4),\ \dots .$$
Let C be the point given by $\overline{OC}=\frac{1}{3}(\alpha_1+\alpha_2+\alpha_3+\alpha_4)$.

Then $\overline{CG_1}=-\frac{1}{3}\alpha_1$, $\overline{CG_2}=-\frac{1}{3}\alpha_2$, $\overline{CG_3}=-\frac{1}{3}\alpha_3$, $\overline{CG_4}=-\frac{1}{3}\alpha_4$.

Hence G_1, G_2, G_3, G_4 lie on a sphere with centre C and radius $\frac{1}{3}R$.

For the general simplex in $[n]$ we clearly get an analogous result, with the centre of the " sphere " given by $(\alpha_1+\alpha_2+\dots+\alpha_{n+1})/n$ and radius equal to R/n.

6.2. Suppose we take 5 points A_1, A_2, ... A_5 on the sphere in [3]. Then to each four we get a sphere like that of § 6.1.

The centres will be C_1, C_2, ... where
$$\overline{OC_1}=\tfrac{1}{3}(\alpha_2+\alpha_3+\alpha_4+\alpha_5),\quad \overline{OC_2}=\tfrac{1}{3}(\alpha_1+\alpha_3+\alpha_4+\alpha_5),\ \dots .$$
Hence, if we take $\overline{OC'}=\frac{1}{3}(\alpha_1+\alpha_2+\dots+\alpha_5)$, we have
$$\overline{C'C_1}=-\tfrac{1}{3}\alpha_1,\quad \overline{C'C_2}=-\tfrac{1}{3}\alpha_2,\dots \overline{C'C_5}=-\tfrac{1}{3}\alpha_5.$$
Thus C_1, C_2, ... C_5 lie on a sphere, Γ', centre C' and radius $\frac{1}{3}R$.

Now take 6 points on the original sphere.

We get 6 spheres such as Γ', each corresponding to 5 of the points.

The centres C_1', C_2', ... are given by
$$\overline{OC_1'}=\tfrac{1}{3}(\alpha_2+\alpha_3+\dots+\alpha_6),\quad \overline{OC_2'}=\tfrac{1}{3}(\alpha_1+\alpha_3+\dots+\alpha_6),$$
$$\overline{OC_3'}=\tfrac{1}{3}(\alpha_1+\alpha_2+\alpha_4+\alpha_5+\alpha_6),\ \dots .$$

Hence if we take $\overline{OC''} = \frac{1}{3}(\alpha_1 + \alpha_2 + \dots + \alpha_6)$, we have

$$\overline{C''C_1'} = -\tfrac{1}{3}\alpha_1, \quad \overline{C''C_2'} = -\tfrac{1}{3}\alpha_2, \quad \overline{C''C_3'} = -\tfrac{1}{3}\alpha_3, \dots .$$

Thus $C_1', C_2', \dots C_6'$ lie on a sphere Γ'', with centre C'' and radius $\frac{1}{3}R$.

And so on.

A similar chain clearly holds in space of n dimensions; in two dimensions we have, to start with, a circle, an inscribed triangle ABC and the nine-point circle of ABC.

Thus if A, B, C, D are four points on the first circle, the nine-point circles of BCD, CDA, ... have their centres on a circle of radius $\frac{1}{2}R$.

When a fifth point is taken on the original, we have five circles like the last one; their centres lie on another circle, also of radius $\frac{1}{2}R$.

And so on. H. L.

1040. Si j'étois maître de toute la gloire que les sçavans se sont efforcé de répandre sur la tête des monarques, en leurs dédiant leurs plus excellens ouvrages, je n'y trouverois rien de plus brillant pour offrir à VOTRE MAJESTÉ que le traité de la Quadrature du Cercle. Ce Cercle, SIRE, est une illustre couronne pour laquelle les plus grands Roys du Monde ont eu de l'ambition. . . . En effet, SIRE, tous ces grands exploits que VOTRE MAJESTÉ a fait aux yeux de l'Europe sur le Rhein & sur le Danube qui restent encore ensanglantés du sang de ses ennemis, seroient un jour ensevelis dans le corps d'une longue histoire parmi les actions d'un grand nombre de héros qui se sont signalés contre elle & pour elle, si ce n'étoit que considérant cette époque de la Quadrature qui lui est dédiée, & dont les siècles parleront, l'on y découvrira les hauts faits & la gloire de son auguste nom, avec une distinction qui la séparera du reste des mortels ; . . .—Dedication of *Le dénouement de la Quadrature du Cercle* par le Sieur de la Motte, Mathématicien (1700). [Per Prof. E. H. Neville.]

1041. In the behaviour of the spring-supported mass there is something almost human ; it objects to being rushed. If coaxed gently and not hurried too much, it responds with perfect docility ; but if urged to bestir itself at more than its normal gait, it exhibits a mulish perversity of disposition. Such movement as it makes under this compulsion is always in a retrograde direction, and the more it is rushed, the less it condescends to move. On the other hand, if it is stimulated with its own natural inborn frequency, it plays up with an exuberance of spirit which may be very embarrassing.—C. E. Inglis, *A Mathematical Treatise on Vibrations in Railway Bridges* (1934). [Per W. G. Bickley.]

1042. Ausserdem sind uns plötzlich Zweifel jekommen. Wir haben uns . . . des Zweiersystems des grossen Leibniz erinnert und dabei entdeckt, dass das ganze Einmaleins dieses Systems, das ja nur die 0 und die 1 als Ziffern kennt, aus einem einzigen Ansatz, nämlich $1 \times 1 = 1$, besteht. Für Schüler ist ein solches Einmaleins sicherlich verlockend. Wir aber sind sehr verwirrt. Denn wir haben behauptet, man könne in jedem beliebigen System nach denselben Regeln rechnen. Wie soll ich aber multiplizieren, wenn ich nur weiss, dass 1 mal 1 gleich 1 ist ?—E. Colerus, *Vom Einmaleins zum Integral* (1934), p. 43. [Einmaleins = multiplication table.] [Per Prof. E. H. Neville.]

A GENERAL METHOD OF DRAWING BENDING MOMENT DIAGRAMS.

By T. W. Hall.

In a previous article (October 1933) it was shown how the "projection method" could be applied to the drawing of bending moment diagrams for fixed concentrated loads. It is proposed to extend this method to cases where there are uniformly distributed loads in addition to the concentrated ones.

The construction depends upon the theorem that any two tangents to a parabola are cut in inverse proportion by a third. Thus in Fig. 1, the tangents AB and AC to the parabola are cut by the

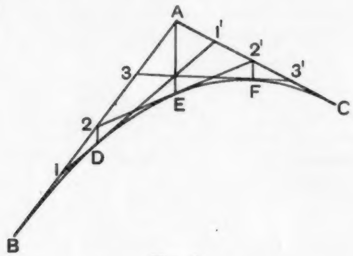


Fig. 1.

tangent 11′ so that $B1 : 1A = A1' : 1'C$. (This is proved in all standard works on Geometrical Conics, *e.g.* C. Smith, *Geometrical Conics*, p. 62.) From it we get a convenient method of drawing a parabola to touch two intersecting straight lines at given points. Let AB and AC be the given lines and B and C the given points. Divide AB and AC into the same number of equal parts and number the points of division from B to A and from A to C. Join 1 to 1′, 2 to 2′, 3 to 3′, etc.: the required parabola is the envelope of the lines. Where the direction of the axis is known, as in the drawing of bending moment diagrams, the points of contact of any tangent may be obtained by drawing lines parallel to the axis of the curve through the intersection of the two adjacent tangents, as $2D$, AE and $2'F$ in Fig. 1.

As an example, a beam loaded with two different intensities and having in addition two concentrated loads is worked out in Fig. 2. The moments required for the construction in (*c*) may be calculated or obtained graphically by the projection method as in (*b*). In Fig. 2 the latter method has been adopted, the moments in (*c*) being taken from (*b*) and enlarged in the ratio of 5 : 1. The distributed loads are considered in sections or "fields" lying between concentrated forces (including, in the case of a beam which is not supported at the ends, the reactions at the supports) or points where the intensity of the distributed load alters; in other words, between points at which there is discontinuity of the bending moment curve, as at

R, *S* and *T*. Parabolas are described by the method of Fig. 1 between *R* and *S* and *S* and *T*.

It is important to be able to find the maximum bending moment for a given loading. A simple method of doing so is shown in Fig. 3.

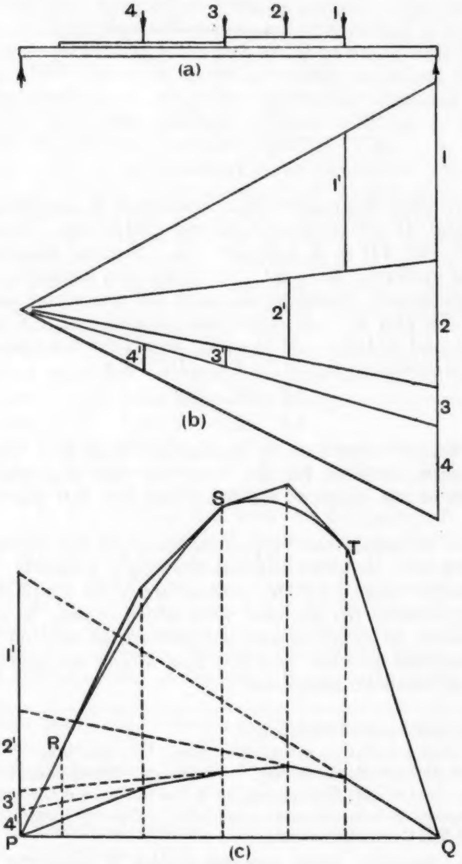


FIG. 2.

If *AB* and *AC* are the tangents, draw *AD* vertically and *BD* horizontally to meet in *D*. Draw *DE* parallel to *BA* to meet *AC* in *E*. Then the horizontal through *E* touches the parabola. Let it cut *BA* in *F*. To find the point of contact, draw *FG* verti-

cally to cut BD in G, and along FE set off FH equal to BG. Then H is the point of contact and is the summit of the ordinate giving the maximum bending moment.

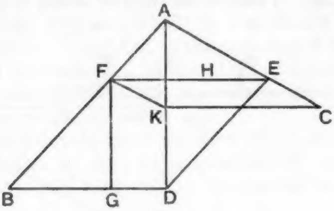


Fig. 3.

To prove that this construction is correct, it is sufficient to show that AB and AC are divided in inverse proportion. Draw CK horizontally to cut AD in K and join FK. Because the load between B and C is uniform, $BD=KC$; they are also parallel. But $BDEF$ is a parallelogram, therefore BD and FE are equal and parallel; therefore FE and KC are equal and parallel and FK and EC are also equal and parallel. It is easily seen that the triangles AFK, DEA are similar, having $AF : FK = DE : EA$.

Thus $$AF : EC = BF : EA$$

or $$AF : FB = CE : EA,$$

so that FE is a tangent to the parabola. That H is the summit of the maximum ordinate follows from the fact that the horizontal projections of two tangents are equal and that FH was made equal to BG.

It should be noted that by this construction the maximum bending moment may be found without drawing a parabola.

Advantages claimed for the method are (i) its simplicity for such a general problem, (ii) the ease with which it may be adapted for a combination of graphics and calculation, as well as for a completely graphical solution, and (iii) that it does not involve the preparation of parabolic templates. T. W. H.

1043. Bullet-Proof Waistcoats.

" It will stop a bullet at point-blank range ", I was told. " But, what is more, it will stop a bullet at twenty yards' range. Fired point-blank, a bullet loses a good deal of its effectiveness, as it has not gained the velocity. That is where assassins sometimes make a mistake."—*Sunday Pictorial*, October 14, 1934. [Per Mr. C. A. Richmond.]

1044. He [Macaulay] would pass one evening in comparing the average duration of the lives of Archbishops, Prime Ministers, and Lord Chancellors; and another in tracing the careers of the first half-dozen men in each successive Mathematical Tripos, in order to ascertain whether, in the race of the world, the Senior Wrangler generally contrived to keep ahead of his former competitors.—Sir G. O. Trevelyan, *Life and Letters of Lord Macaulay* (Popular Edition; Longmans, 1889), p. 666.

CORRESPONDENCE.

THE SOLUTION OF TRIANGLES GIVEN THREE SIDES.

To the Editor of *The Mathematical Gazette*.

DEAR SIR,—The report in your July issue (Vol. XIX, No. 234, p. 180) of the paper read by Mr. Hope-Jones at the Annual Meeting of the Mathematical Association contains several points of interest.

Everybody will agree on the importance of accuracy in arithmetic, for without it the work is in practice a waste of time; and those who took part in the discussion gave interesting methods by which numerical applications of one or two particular formulae could be checked. This raises the question whether each formula is to be provided with its own set of checks. Obviously this is impossible; so why single out special ones and leave the rest to take care of themselves? It is certain that students will neither remember nor use these methods in later life when they may have to use the formulae. Some general method of ensuring the accuracy of all numerical work is urgently needed, and would be of the utmost value.

The second point I wish to mention is the desirability of realizing and of stating clearly that four-figure tables do not, as a rule, enable results accurate to four figures to be obtained. It follows that when such tables are used in trigonometrical formulae angles correct to the nearest minute must not be expected. The use of half-minutes is to be deprecated strongly as suggesting a degree of accuracy which does not in fact exist.

It is an interesting exercise to write down at random a set of numbers of four figures, to use four-figure tables to find their logarithms, and then to check the results by using the table of antilogarithms. I have just done this with ten numbers; in five cases there was disagreement in the last figure. In practical numerical work this is the kind of accuracy that arises. It may perhaps be asked here whether there is any valid reason why occasionally five-figure and seven-figure tables should not be used in schools. It is often necessary to use them in practical problems to obtain the required degree of accuracy. The four-figure table fetish of modern times seems almost as bad as the seven-figure table fetish of former generations.

And one last point. How does a mathematician justify the use of such a statement as $\log 2R = 3{\cdot}0523\frac{1}{2}$?

To use such a notation is bad, but surely to teach it is worse.

Yours faithfully,

The Royal Technical College, R. O. STREET.
Glasgow, 2nd October 1935.

DEAR SIR,—(1) *If we cannot find good checks for all formulae, why use them for any?*

If we cannot learn all languages, why learn any?
If we cannot clear all slums, why clear any?
If we cannot marry all women, why marry any?

(2) *Results obtained by using four-figure tables are frequently incorrect in the fourth figure.* We all realize this ; but is a twenty-minutes address to an audience which presumably knows it already the best time for clearly stating it ? In an article on Probability and Approximations published in 1917-8, (*M.G.* Nos. 127, 130, 133), of such portentous stodginess that nobody except the author ever read to the end of it, I devoted about a quart of ink to the fourth figure's hopes of salvation : am I to regurgitate it all whenever I make an approximate calculation in public, and risk the casualties involved in the ensuing rush for the door ?

Has Mr. Street noticed the differences which make for the uncertainty of the fourth figure in logarithmic multiplication, and how widely they differ in magnitude from the corresponding differences in the table of logarithmic tangents ? Taking both tables at their weakest point, a variation in the logarithm which would alter the fourth figure of a numerical answer by 1 alters an angle found from its log tan by ·17 of a minute.

(3) *The use of half-minutes* ensures a higher degree of accuracy than that obtained without them. Is more justification needed ? If it is; notice that without them we can *never* get answers correct to the nearest minute if at least one of the right answers is odd, which happens thirteen times out of sixteen.

(4) *Anti-logarithms.* Poison.

(5) *Five-figure and seven-figure tables in schools.* Most of us make opportunities of showing such tables to boys, so as to dissipate the idea that logarithms are necessarily four-figured. To provide all boys with such tables is a more difficult matter. And did I omit all use of seven-figure tables in the offending discussion ?

(6) " *Log* $2R = 3{\cdot}0523\frac{1}{2}$," or, to quote my own words, log cos $\frac{1}{2}A = \bar{1}{\cdot}5758\frac{1}{2}$. " *To use such a notation is bad.*"

According to the first definition of an index that we learn, x^0 and $x^{2\frac{1}{2}}$ have no meaning. But because they are convenient, we extend the definition so as to bring their behaviour into line with that of x^1 and x^3. $8\frac{1}{2}$ was not included in the first meaning of " number " which we learnt : later on we admitted it to fellowship with its neighbours 8 and 9. Then why not admit it as the last figure of an approximate decimal, by a simple extension of the definition ? I am no lover of Americanisms, but I take no offence at the regular abbreviation \$4·90$\frac{1}{4}$, which is both clear and short. Notation was made for man, not man for notation.

$\bar{1}{\cdot}5758$ means " minus one, plus five tenths and seven hundredths and five thousandths and a number of ten-thousandths of which the central and most probable value according to the evidence at our disposal is eight." $\bar{1}{\cdot}5758\frac{1}{2}$ means the same thing with " eight and a half " substituted for " eight." Am I to write all this stuff every time ? Or am I to call it $\bar{1}{\cdot}57585$ and go hunting for it among the four-figured inhabitants of a four-figure table, with the additional burden on my conscience of knowing that the 5 in the fifth place is very unlikely to be the true figure ?

(7) "*But surely to teach it is worse.*" If, as I believe, there are dangers in this notation for beginners, the best way to start is this: "When you halve 7 in the last figure, writing $3\frac{1}{2}$ is a thing that some people object to: so call it $3+$ or $4-$ and treat it as $3\frac{1}{2}$ when you look for it in the table." But the best boys pave the way for the next step by resenting the hypocrisy of calling it one thing and treating it as another.

Yours faithfully,

Eton, 9th October 1935. W. HOPE-JONES.

DEAR SIR,—Browsing recently, during a period of enforced idleness, among the *Reports* of the Association for the Improvement of Geometrical Teaching, I was amused to find, on p. 12 of the XVIIIth Report (1892), a tabulation for the work involved in "the Solution of Triangles, given three sides," which is substantially the same as the excellent one given at the beginning of the Appendix to a paper bearing that title in a recent issue of the *Mathematical Gazette* (Vol. XIX, p. 187—July 1935).

It is at once amusing and comforting that one of the results of the paper and discussion on this topic, which gave many of us a pleasant hour last January, and of the collaboration which succeeded, should show such close agreement with the work of forty-three years ago. Equally comforting must it be for the author of the 1892 paper—Prof. Alfred Lodge—to find his methods thus unwittingly and independently confirmed by a later generation.

The three particulars in which the methods differ are as follows:—

(1) Prof. Lodge writes $\log s$ under s at the bottom of the second column, and where the 1935 method first inserts the logarithm of s he inserts $\operatorname{colog} s$ (i.e. $\log 1/s$) and so obtains $\log r^2$ by the addition of four logarithms instead of by adding three and subtracting a fourth.

(2) The final column (angles) is absent from the earlier method, the total of the half-angles being used as the final check. In spite of this,

(3) the earlier method is not so well compressed as the later one, being printed *along the length* of the page.

Some of those early *Reports* of our Association make very interesting reading; it is a pity they are so inaccessible to most of us. We might do worse than devote a portion of the *Gazette* each year to the reprinting of judiciously selected extracts from some of the papers and discussions of fifty years ago. Parts of them are surprisingly up-to-date. In particular, a paper read in 1889 by Prof. G. M. Minchin, entitled "The Vices of our Scientific Education", would bear reprinting as a whole. There is something encouraging in reading of a few vices which are now dying out, and a more doubtful encouragement in realising that other familiar ones were causing an earlier generation as much anxiety as they do ours.

Yours faithfully,

11th November 1935. J. T. COMBRIDGE.

MATHEMATICAL NOTES.

1171. *The tractrix in the design of loud-speakers.*

Loud-speakers have recently been made with side elevation in the form of a double tractrix, the asymptote of each curve coinciding with the axis of the speaker. The cross-section may be circular or square. The advantage of using this shape is perhaps due to the fact that the tractrix is the orthogonal trajectory of a series of equal circles.

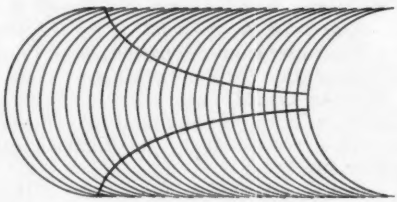
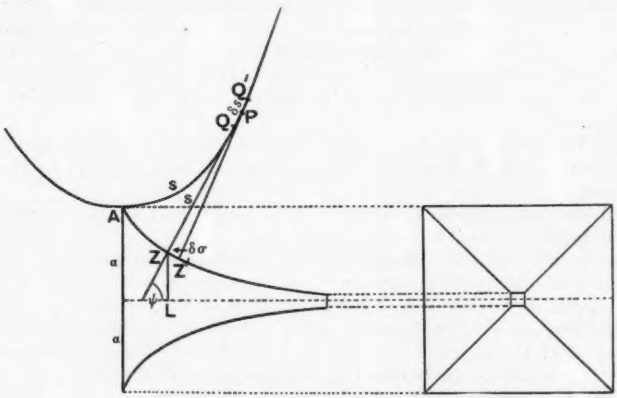


FIG. 1.

It was proposed to make a speaker having a side elevation as in Fig. 2, with a square cross-section, and the question was asked, what should be the shape of the four pieces which when bent would form the sides of such a speaker ?



Side elevation. Front elevation.

FIG. 2.

Let σ be the length of the tractrix from its vertex A to a point Z, $\delta\sigma$ being the small increment ZZ'. The normals at Z, Z' will touch the catenary $s=a \tan \psi$ at Q, Q', suppose, intersecting one another at a point P.

$$ZQ = \text{arc } AQ = s.$$
$$Z'Q' = \text{arc } AQ' = s + \delta s.$$
$$ZPZ' = \delta\psi.$$

Then $d\sigma/d\psi = s = a \tan \psi$, whence $\sigma = a \log \sec \psi$.
The ordinate of Z, ZL, $= a \cos \psi$.

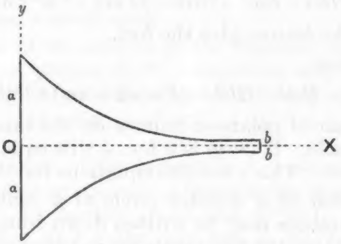


FIG. 3.

The shape of the four pieces is as shown in Fig. 3, the bounding curves being defined by

$$x = \sigma = a \log \sec \psi,$$
$$y = \pm ZL = \pm a \cos \psi,$$

from which

$$-\frac{x}{a} = \log \frac{y}{a} \quad \text{or} \quad y = ae^{-x/a}.$$

The length, measured along the x-axis, is

$$a \log_e (a/b).$$

E. H. LOCKWOOD.

1172. *Note on Complex Geometry.*

In the *Gazette*, XVIII, p. 43, will be found these words: "If the conic is an ellipse, the real eccentricity goes with the real foci and the imaginary eccentricity with the imaginary foci, but if the conic is a hyperbola, there is no such relation . . .". Similar remarks may be found in the older textbooks, and even in some of the modern ones.

Now if the geometry is complex, there is no distinction between ellipse and hyperbola: conics are parabolas or central conics. If, on the other hand, the geometry is real, an ellipse or hyperbola only has two foci.

To what geometry, then, do the above words refer ? A. R.

1173. *Note on Foci.*

In two recent articles in the *Gazette* (XVIII, p. 43, and XIX, p. 87) it is shown how the foci may be found from the general locus equation of a conic. Part of the object of those articles was to correlate methods of dealing with foci, directrices, eccentricities, and axes.

The purpose of the present note is different. It is to suggest that

foci ought *not* to be found from the general locus equation. The foci are point-pairs of the four-line system of conics determined by the given conic and the circular points. Two or three methods of determining them analytically are suggested by this fact. For example :

(1) λ may be chosen so that

$$Al^2+2Hlm+Bm^2+2Gln+2Fmn+Cn^2+\lambda(l^2+m^2)$$

factorises, and the factors give the foci.

(2) The equation

$$Al^2+2Hlm+Bm^2-(2Gl+2Fm)(lx+my)+C(lx+my)^2=0$$

represents the pair of points at infinity on the tangents from (x, y) to the general conic. If (x, y) is a focus, this equation must be the same as $l^2+m^2=0$. This gives the equations for the foci.

(3) The equation of a director circle of a central conic or the directrix of a parabola may be written down from the equation in (2) by expressing that the two points are in perpendicular directions. Also, the pairs of directrices of a central conic are the parabolas of the four-point system determined by the conic and director circle.

It is not intended to imply here that any one particular method is always the best, but only to suggest that in dealing with foci there is no good reason for using the locus equation instead of the tangential. A. R.

1174. *On differentials.*

In Prof. Picken's article on Differentials, *Gazette*, XVIII, p. 82, he says : " The elementary relations of Differential Geometry yield at once to this approach. Thus

$$ds^2=dx^2+dy^2=dr^2+r^2d\theta^2$$

are consequences of the ultimate equivalence of arc-element and chord-element . . . and from the same infinitesimal right-angled triangles . . . $ds=dx/\cos\psi$, etc.—all with immediate ' reference back ' to the corresponding ' increment-infinitesimals ' (and to ultimate equivalence)."

There is a danger that this paragraph may be misleading. The following extract from Prof. R. H. Fowler's tract (Cambridge Tracts : No. 20) shows the sense in which the words must be read.

" Any arc of a plane curve which has a continuous tangent is *rectifiable*, and the length of the arc measured from a point P to a point Q (the coordinates being rectangular cartesians) is given by

$$s=\int_{x_0}^{x}\{1+[f'(x)]^2\}^{\frac{1}{2}}\,dx, \qquad (2{\cdot}51)$$

. . . using differentials we obtain

$$ds^2=dx^2+dy^2. \qquad (2{\cdot}531)$$

Note that 2·531 *is not the source of* 2·51, *but a deduction therefrom.* Equation 2·51 is fundamental, for it is a direct deduction from the

definition of the length of an arc as the limit of an inscribed polygon, and until 2·51 has been established no meaning can be attached to 2·531."

It would also seem desirable in reading Section 9 of Prof. Picken's article to bear in mind the paragraph following 5·312 in Prof. Fowler's tract (p. 67).

A. ROBSON,
C. V. DURELL.

1175. *On special roots of unity.*

The treatment of the subject in the standard work by Burnside and Panton is not perfect (§ 53). Attention may be drawn to the defective proof of the well-known result that *if* α *and* β *be special roots of* $x^{p^a}=1$ . . . (*i*), *and of* $x^{q^b}=1$ . . . (*ii*), (p, q *are primes*), *then* $\alpha\beta$ *is a special root of* $x^n=1$ . . . (*iii*), *where* $n=p^a \cdot q^b$. In the proof given, no use is made of the facts that α, β are special roots of (i) and (ii). This arises on account of ignoring the possibility of having $(\alpha\beta)^m=1\,(m<n)$, by having $\alpha^m=1$ and $\beta^{-m}=1$. Clearly, since p^a and q^b are coprime, this is the only way in which α^m can be equal to β^{-m}.

Now, since α is a special root of (i), $\alpha^m=1$ only if m be a multiple of p^a, and similarly $\beta^{-m}=1$, *i.e.* $\beta^m=1$, if m be a multiple of q^b. Hence, if both $\alpha^m=1$ and $\beta^{-m}=1$, m is to be a multiple of both p^a and q^b, which is impossible if $m<n$. This completes the proof.

Again, in saying that the number of special roots of $x^n=1\,(n=p^aq^b)$ is $n\left(1-\dfrac{1}{p}\right)\left(1-\dfrac{1}{q}\right)$, the authors do not show that the combination $\alpha\beta$ where at least one of the factors—say the second—is a non-special root, is not a special root of (iii). The proof is simple ; but the statement is important. $\beta^\kappa=1\,(\kappa<q^b)$, and $\alpha^{p^a}=1$. Hence $(\alpha\beta)^{\kappa p^a}=1\,(\kappa p^a<n)$.

Thus a special root of (iii) arises only by combining a special root of (i) with a special root of (ii), and hence the number is

$$p^a\left(1-\frac{1}{p}\right)q^b\left(1-\frac{1}{q}\right) \quad or \quad n\left(1-\frac{1}{p}\right)\left(1-\frac{1}{q}\right).$$

D. N. S.

1176. *The quadratic quotient.*

The following is, I believe, a somewhat different treatment of the solution of a very old problem, namely, to determine the range of possible values of the quadratic quotient for real values of the variable.

Let $$(ax^2+bx+c)/(a'x^2+b'x+c')\equiv y.$$

Then $$x^2(a-a'y)+x(b-b'y)+(c-c'y)=0,$$

and $$\{2x(a-a'y)+(b-b'y)\}^2=(b-b'y)^2-4(a-a'y)(c-c'y)$$
$$=(b^2-4ac)-2y(bb'-2ac'-2a'c)+y^2(b'^2-4a'c'). \quad \text{...(i)}$$

The variation of treatment lies in a transformation of equation (i) which, though somewhat artificial, pays handsomely in the end.

Let $\quad bc' - b'c = A, \quad ca' - c'a = B, \quad ab' - a'b = C$;

then $\quad aA + bB + cC = 0, \quad a'A + b'B + c'C = 0.$

Also
$$(b^2 - 4ac)C^2 = b^2C^2 + 4aC(aA + bB)$$
$$= (bC + 2aB)^2 - 4a^2(B^2 - AC). \quad \text{.........(ii)}$$

Similarly $\quad (b'^2 - 4a'c')C^2 = (b'C + 2a'B)^2 - 4a'^2(B^2 - AC). \quad \text{......(iii)}$

And
$$(bb' - 2ac' - 2a'c)C^2 = bb'C^2 + 2aC(a'A + b'B) + 2a'C(aA + bB)$$
$$= (bC + 2aB)(b'C + 2a'B) - 4aa'(B^2 - AC).$$

Hence on multiplying equation (i) throughout by C^2, we have
$$\{2x(a - a'y)C + (b - b'y)C\}^2$$
$$= \{(bC + 2aB) - (b'C + 2a'B)y\}^2 - 4(B^2 - AC)(a - a'y)^2. \text{....(iv)}$$

I. If $B^2 - AC$ is negative, so that the right-hand side of equation (iv) is positive, y can assume all values for real values of x. From (ii) and (iii) it follows that $b^2 - 4ac$ and $b'^2 - 4a'c'$ are both positive, so that the roots of $ax^2 + bx + c = 0$ and $a'x^2 + b'x + c' = 0$ are all real. The condition $B^2 - AC < 0$ further shows that the roots "interlace"; *vide* Chrystal, *Algebra*, I, 464-5.

II. If $B^2 - AC$ is positive, $= \Delta^2$, say, we have that the right-hand side of equation (iv) is
$$\{(bC + 2aB + 2a\Delta) - (b'C + 2a'B + 2a'\Delta)y\} \times$$
$$\{(bC + 2aB - 2a\Delta) - (b'C + 2a'B - 2a'\Delta)y\}.$$

Now by (iii) the coefficients of y in the above factors are of the same sign or of opposite signs according as $b'^2 - 4a'c'$ is positive or negative.

Hence, for real values of x, the values of y are wholly excluded from or wholly included within the interval
$$\frac{bC + 2aB + 2a\Delta}{b'C + 2a'B + 2a'\Delta} \text{ to } \frac{bC + 2aB - 2a\Delta}{b'C + 2a'B - 2a'\Delta},$$
according as the roots of $a'x^2 + b'x + c' = 0$ are real or imaginary.

In the former case we may find a formula for x which gives the intermediate values. We have, from (iv),
$$\{2x(a - a'y)C + (b - b'y)C\}^2$$
$$= \{(b - b'y)C + 2(a - a'y)B\}^2 - 4(B^2 - AC)(a - a'y)^2,$$
or
$$\left\{2Cx + \frac{b - b'y}{a - a'y}C\right\}^2 = \left\{\frac{b - b'y}{a - a'y}C + 2B\right\}^2 - 4(B^2 - AC).$$

Putting
$$\frac{b - b'y}{a - a'y}C = -2B - 2\Delta\cos\theta,$$
which, for $0 < \theta < \pi$, gives all the values between $-2B \mp 2\Delta$, we at once obtain
$$Cx = B + \Delta(\cos\theta + i\sin\theta),$$

while the corresponding values of y are

$$\frac{bC+2a(B+\Delta\cos\theta)}{b'C+2a'(B+\Delta\cos\theta)}.$$

N. M. GIBBINS.

1177. *Solution of the triangle, given a, b, A. (The "ambiguous case.")*

The discussion of this case in most of the textbooks of trigonometry in common use is somewhat imperfect, and the methods employed might be improved upon. The following procedure is preferable.

$$\operatorname{Sin} B=\frac{b}{a}\sin A.$$

Three cases. (1) $\frac{b}{a}\sin A>1$. Triangle impossible.

(2) $\frac{b}{a}\sin A=1$. Then $B=90°$.

(3) $\frac{b}{a}\sin A<1$, say $\frac{b}{a}\sin A=\sin B_1$.

In case (3) $B=B_1$ or $180°-B_1\equiv B_2$.

Then $C_1=B_2-A,\quad C_2=B_1-A.$

Here several cases may arise.

(α) $C_1<0,\quad C_2<0$. Triangle impossible.

(β) $C_1>0,\quad C_2<0$. Only one solution, $C=C_1$.

(γ) $C_1>0,\quad C_2>0$. Two solutions.

Going back to case (2), $C=90°-A$. There will be one solution or none according as $90°-A\gtrless 0$, that is, according as A is acute or obtuse.

Thus (3) (γ) is the only case in which there are two solutions.

The angles having been determined, the remaining side is got, of course, by $c=(a\sin C)/\sin A$. The appropriate treatment by logarithms is obvious. Note merely that the order

$$\log\sin B=\log b-\log a+\log\sin A$$

is more economical than

$$\log\sin B=\log b+\log\sin A-\log a,$$

as the latter order requires one to turn over the leaves of the logarithm book four times instead of twice! That is, if only one table of logarithms is used.

The gist of the above method lies in the use made of the *signs of C_1 and C_2*. Most of the textbooks I have consulted slur over the case when A is obtuse.

R. F. MUIRHEAD.

1178. *The number of primes.*

The under-mentioned scrap of information may be of interest to those readers who are concerned with the various formulae of eminent mathematicians for the number of primes less than a given prime.

Completion of a Factor Table for the interval 100 millions to 100 millions 100 thousand shows that in this interval there are 5413 primes : this result may be compared with those for intervals of equal magnitude given in Glaisher's Factor Tables of which a few are appended.

Existing Factor Tables only go as far as 10 millions, and the isolated case dealt with is to be regarded only as presenting an opportunity of comparing the results of extrapolation from the formulae with a definite ascertained fact.

GLAISHER : NUMBER OF PRIMES COUNTED.

Interval.	*No. of Primes.*
0 to 100,000	9593
1,000,000 „ 1,100,000	7216
2,000,000 „ 2,100,000	6874
3,000,000 „ 3,100,000	6676
4,000,000 „ 4,100,000	6628
5,000,000 „ 5,100,000	6458
6,000,000 „ 6,100,000	6397
7,000,000 „ 7,100,000	6369
8,000,000 „ 8,100,000	6250
8,900,000 „ 9,000,000	6270

HAMILTON KILGOUR.

1179. *A property of the ellipse.*

AA' is the major axis of an ellipse, centre O, eccentricity e, and also a diameter of a circle. If the angle between the planes of the two curves is arc sin e, the ellipse will be the orthogonal projection of the circle. Let S be a focus, P a point on the ellipse, P' the corresponding point on the circle, N the foot of the perpendiculars from P and P' on AA'. Then, with the usual notation,

$$SP=a+e\,.\,ON.$$

Take a point C on SP such that $SC=a$. We have

$$CP'^2=CP^2+PP'^2,$$

and $$CP=e\,.\,ON,\quad PP'=e\,.\,P'N.$$

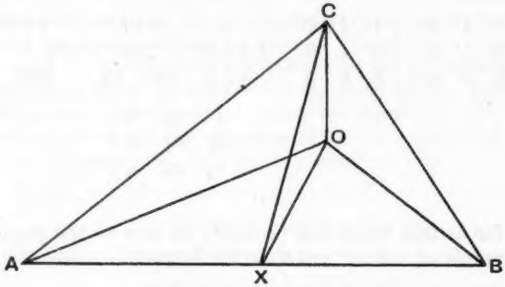
Thus $$CP'^2=e^2(ON^2+P'N^2)$$
$$=e^2a^2.$$

Thus if the plane SPP' rotates about an axis through S perpendicular to the plane of the ellipse, the locus of P', in the moving plane, will be a circle centre C and radius ae. It follows that if the circle be rotated about the same axis it will generate an anchor ring.

This problem was suggested by the well-known property of an anchor ring that a tangent plane through the centre cuts the surface in two circles. J. LISTER.

1180. *Two proofs of Euclid XI*, 4.

Let OC be perpendicular both to OA and to OB, and let OX be any other line through O in the plane AOB.



Through X draw AXB in such a way that $AX=XB$. Then

$$\begin{aligned} 2(AX^2+CX^2) &= AC^2+BC^2 \\ &= (OA^2+OC^2)+(OB^2+OC^2) \\ &= OA^2+OB^2+2\,.\,OC^2 \\ &= 2(AX^2+OX^2+OC^2). \end{aligned}$$

Hence $$CX^2=OX^2+OC^2,$$

and so COX is a right angle.

Otherwise.

It is easily proved that if X is any point in the base AB of an isosceles triangle OAB, then

$$OA^2=OX^2+AX\,.\,XB.$$

Now let OC be perpendicular both to OA and to OB, and let OX be any other line through O in the plane AOB. Through X draw AXB equally inclined to OA and OB, so that $OA=OB$. Then the triangles COA, COB are congruent. Thus X is a point in the common base AB of two isosceles triangles OAB, CAB.

Hence $$OA^2=OX^2+AX\,.\,XB,$$

and $$CA^2=CX^2+AX\,.\,XB.$$

Thus $$CA^2-OA^2=CX^2-OX^2,$$

or $$CO^2=CX^2-OX^2,$$

and so COX is a right angle. W. J. DOBBS.

1181. *On Note* 1146 (*Gazette*, XIX, 224).

My former headmaster, Mr. L. Sadler, has been showing his pupils the determinantal method of finding the condition that the

conic $ax^2+2hxy+by^2+2gx+2fy+c=0$ represents two straight lines for many years now. He attacks another problem with the same weapon.

To find the condition that the four points given by

$$S \equiv x^4+4a_1x^3+6a_2x^2+4a_3x+a_4=0$$

are harmonic.

Let $S \equiv (x^2+2hx+b)(x^2+2h'x+b')$, then the product of the zero determinants:

$$\begin{vmatrix} 1 & 1 & 0 \\ h & h' & 0 \\ b & b' & 0 \end{vmatrix} \begin{vmatrix} 1 & 1 & 0 \\ h' & h & 0 \\ b' & b & 0 \end{vmatrix} = \begin{vmatrix} 2 & h+h' & b+b' \\ h+h' & 2hh' & bh'+b'h \\ b+b' & bh'+b'h & 2bb' \end{vmatrix}$$

$$=8\begin{vmatrix} 1 & a_1 & a_2 \\ a_1 & a_2 & a_3 \\ a_2 & a_3 & a_4 \end{vmatrix}$$

$$=0.$$

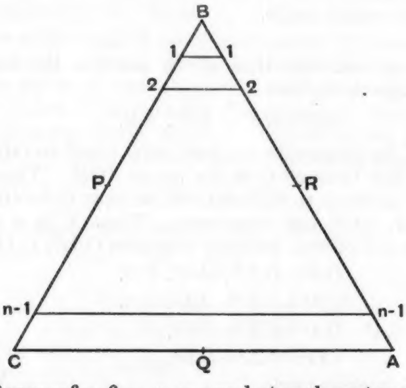
In so far as this result will probably be new to the pupil, it is a better example of the method than the former. RONALD FRITH.

1182. *Answer to "A Challenge" (Note 1129).*

Let ABC be the nth equilateral triangle and let AP, BQ and CR be the three medians. Let CA and CB be taken as axes of x and y. Then the equations of the six systems of lines parallel to BC, CA, AB, AP, BQ and CR respectively can be written in the form

$$x=\alpha, \quad y=\beta, \quad x+y=\gamma, \quad x+2y=p, \quad 2x+y=q, \quad y-x=r.$$

The coordinates of the vertices of the several triangles can now be



expressed in terms of $\alpha, \beta, \gamma, p, q, r$ and at each vertex we must have $x \geqslant 0$, $y \geqslant 0$, $x+y \leqslant n$. Now let $\beta\gamma p$ denote the triangle formed by the lines $y=\beta$, $x+y=\gamma$, $x+2y=p$ and let $N(\beta\gamma p)_n$ denote the number of triangles of this type which are within ABC, the figure

made up of the first n tiers. Then it can easily be proved that the total number of triangles in the first n tiers is

$$N(\alpha\beta\gamma)_n + 3N(\beta\gamma p)_n + 6N(\beta\gamma q)_n$$
$$+ 3N(pq\gamma)_n + 6N(qr\gamma)_n + N(pqr)_n.$$

To obtain formulae for these terms we use the method of differences ; thus :

$$N(\alpha\beta\gamma)_n - N(\alpha\beta\gamma)_{n-1}$$

is the number of triangles $\alpha\beta\gamma$ which lie *partly* or *wholly* in the nth tier, *i.e.*, the trapezium below the line $(n-1)-(n-1)$. Considering the position of the lowest vertex or the lowest side of these triangles we can calculate their number with the help of the inequalities mentioned above and obtain a difference equation for $N(\alpha\beta\gamma)_n$. Difference equations for the other terms in the expression may be obtained in the same way, and solving these we find

$$N(\alpha\beta\gamma)_n = \{2n^3 + 5n^2 + 2n - (0 \text{ or } 1)\}/8,$$
according as n is even or odd ;

$$N(\beta\gamma p)_n = \{2n^3 + 3n^2 - 2n - (0 \text{ or } 3)\}/12,$$
according as n is even or odd ;

$$N(\beta\gamma q)_n = \{5n^3 + 12n^2 + 3n - (0, 2 \text{ or } 4)\}/18,$$
according as $n \equiv 0, 1, 2 \pmod{3}$;

$$N(pq\gamma)_n = \{14n^3 + 33n^2 + 4n - (0, 3, 12 \text{ or } 15)\}/48,$$
according as $n \equiv 0, 1, 2, 3 \pmod{4}$;

$$N(qr\gamma)_n = a_n + b_n$$

where $a_n = \{2n^3 + 3n^2 - 3n - (0, 2, 2, 2 \text{ or } 4)\}/10,$
according as $n \equiv 0, 1, 2, 3, 4 \pmod{5}$;

and $b_n = \{2n^3 + 5n^2 + 2n - (0 \text{ or } 1)\}/8,$
according as n is even or odd ;

$$N(pqr)_n = \{3n^3 - n - (0, 2 \text{ or } 4)\}/3,$$
according as $n \equiv 0, 1 \text{ or } 2 \pmod{3}$.

If we use the notation $[x]$ for the integral part of x then the result may be written in the form :

$$7[\tfrac{1}{8}(2n^3 + 5n^2 + 2n)] + [\tfrac{1}{4}(2n^3 + 3n^2 - 2n)] + 2[\tfrac{1}{3}(5n^3 + 12n^2 + 3n)]$$
$$+ [\tfrac{1}{16}(14n^3 + 33n^2 + 4n)] + 3[\tfrac{1}{5}(2n^3 + 3n^2 - 3n)] + 2[\tfrac{1}{3}(3n^3 - n)].$$

It can be shown that this agrees with the answer given by Mr. Horsley in Note 1162 to Mr. Travers' *Challenge*.

The method explained above also enables us to prove that if each side of a parallelogram be divided into n equal parts and lines are drawn through the points of division, parallel to the sides and diagonals of the parallelogram, then the number of triangles so formed which lie within the parallelogram is

$$\tfrac{1}{2}\{6n^3 + 9n^2 + 2n\} - \tfrac{1}{4}\{1 - (-1)^n\}.$$

P. K. KASHIKAR.

REVIEWS.

Einführung in die neueren Methoden der Differentialgeometrie. I. Algebra und Übertragungslehre. By J. A. Schouten and D. J. Struik. Second edition. Pp. xii, 202. RM. 10; geb. RM. 11.50. 1935. (Noordhoff, Groningen)

The work before us is the *first volume* of the second edition of a book that was published in one volume some years ago. Though appearing under the same title this new edition is, as the authors affirm, really an entirely new book. The volume under consideration is largely the work of Professor J. A. Schouten. It is concerned mainly with the mathematical preliminaries of the subject, developing the calculus employed and the theory of Connections. It takes the place of the first two chapters of the first edition, but contains a great deal more than those, which were confined to manifolds determined by quadratic metrics.

The volume is divided into two Parts (or Chapters), the first of which deals with the Algebraic portion of the subject in five sections, while the second is concerned with the theory of Connections and embraces seven sections. The first section treats the introductory notions of coordinate systems, geometrical objects and transformation groups, and mentions the characteristics of Kleinian geometry. The second section introduces the simple but important case of the affine group, the corresponding affine space being devoted by E_n, where n is the number of independent variables. This section, which is the longest in Part I of the volume, treats such fundamental concepts as invariants, directions, covariant and contravariant vectors, affinors, densities, Hermitian quantities, projection and various types of indices. The section closes with a brief consideration of a few special cases corresponding to certain restrictions on the affine group. The third section is concerned with affinors of valency 2 in E_n and their corresponding matrices. Besides meeting sums and products of affinors we are introduced to the reciprocal, the isomer, the complex conjugate, and the isomer of the complex conjugate. Hermitian, symmetric and alternating affinors are seen to play an important part. The fourth section treats the algebraic geometry of an affine space R_n, which is associated with a fundamental tensor of constant components. The angle between two vectors is defined, and the meaning of the term orthogonal as applied to vectors or to groups of transformations is made clear. In particular infinitesimal orthogonal transformations are discussed, and also the principal directions of a tensor. Part I of the volume is brought to a close in section five, which treats the algebraic geometry of an affine space, U_n, with which is associated as fundamental tensor a covariant Hermitian tensor of order n with constant components. Results are obtained corresponding to those derived in the case of an R_n.

The second part (or chapter) of the volume, dealing with the theory of Connections, begins by introducing the local affine space at any point of an n-dimensional manifold, and then defines the gradient of a scalar field. Systems of reference, both holonomic and anholonomic, are considered, and there is a brief discussion of the Pfaffian problem. The seventh section is devoted to the covariant differential and the covariant derivatives of a vector, along with the idea of pseudoparallel displacement of the vector. The concepts of the divergence and the rotation of a vector field are also introduced. These ideas are further developed in the next section, which treats the general linear connection, as well as metric and half-metric connections, and deduces the conditions that a connection be Riemannian. Section 9 is concerned largely with the connection induced in a sub-space of the manifold considered; and here we are introduced to the D-symbolism of Van der Waerden and Bortolotti, by

means of which the results obtained are expressible in very compact forms. The tenth section is devoted to geodesics, geodesic manifolds and geodesic systems of reference. A geodesic is defined as a line whose direction undergoes a pseudoparallel displacement along the curve: and a geodesic system of reference with pole at P as one in which all the coefficients of the linear connection vanish at P. A proof is given of the existence of holonomic systems of this kind when the connection is symmetric, and of the extended theorem, due to Fermi and Eisenhart, according to which the coefficients of connection can be made to vanish at all points of a given curve. Section 11, which deals with curvature properties, is the largest in this volume, and contains some very interesting geometrical applications. The Riemann-Christoffel curvature tensor is introduced, the identities which it satisfies are proved, and their geometrical significance is discussed. Manifolds of constant Riemannian curvature are considered, as well as a generalisation of the theorem of Stokes. In the final section various problems in small deformations are discussed; and it is proved that a geodesic in V_n is a line whose length is stationary for small deformations of the curve.

This new edition thus represents a much wider treatment of the subject than that contained in the first edition, both in generality and in the number of topics undertaken. Still, as the authors mention in their Preface, they have intentionally limited the range of the work so as to exclude the geometries of Finsler and Berwald, as well as projective and conformal differential geometries. In the matter of notation they have discarded the double formulation of the first edition, and here confine themselves to that of the Ricci Calculus. They also adhere to the principle of representing the same geometrical object always by the same symbol, a change in the system of coordinates being indicated by a change in the type of indices associated with it. They have also introduced the symbols $\overset{*}{=}$ and $\overset{h}{=}$ so as to be able to distinguish clearly the equations which are invariant from those which are not invariant, or not completely so. The sign $=$ is used only in those equations which retain their form when there is a change to anholonomic coordinates.

A large number of important and useful examples are given throughout the text, and their solutions are indicated at the end of the volume. Many of the examples are really additional theorems. There is a Bibliography of ten pages which, though not intended as a complete one, will prove very useful to the reader. An index of seven pages should also be very helpful; and a careful reading of the proofs has made it possible to print a short list of errata at the end of the book. The dedication is very appropriately to Dr. Tullio Levi-Cività, who is one of the outstanding figures in this field of work.

A book, written by two men who have contributed so much to the development of the subject, is obviously a valuable addition to its literature. According to the Preface, the authors believed, or at least hoped, that they were writing an elementary treatise. The word "elementary" is of course a relative term. The book is not suitable for a beginner. But it will be a great help to the student who is already familiar with the elements of the subject and who wishes to extend his knowledge. We confidently recommend it to every mathematician who is interested in Differential Geometry.

C. E. Weatherburn.

Die Pseudosphäre und die nichteuklidische Geometrie. By F. Schilling. Second edition. Pp. vi, 215. Geb. RM. 13.60. 1935. (Teubner)

The pseudosphere, as the simplest surface with a constant negative curvature, holds a fundamental position in the study of non-euclidean geometry. As was first shown by Beltrami in 1868, the euclidean geometry on its surface

is equivalent to the geometry of Lobatschefsky on the plane when the geodesics are read as straight lines in the plane. The surface is usually investigated by the methods of differential geometry as a particular case of surfaces of constant curvature ; it is one of the objects of the book before us to obtain and exhibit the results independent of this technique, keeping in view the analogous results for the sphere and the elliptic geometry on the plane. The first part of the present volume appeared some few years ago. It arose, the author tells us, from a discussion at the physical colloquium of the Hochschule at Danzig and was intended for students at this stage who had already some knowledge of projective and non-euclidean geometry. The second part is more elaborate ; though the methods are still elementary, the investigations are more detailed and the exercises which are solved add to our knowledge of the behaviour of some curves on the surface.

The pseudosphere, formed by the revolution of the tractrix about its asymptote, is immediately mapped on a plane (x, y) with hyperbolic arc-measurement, its geodesics appearing as circles whose centres lie on the x-axis. By dividing up the formulae of mapping into two transformations, the orthogonal projection of a geodesic on the base-plane of the pseudosphere is very simply constructed in two steps. But only points of the map for which y is greater than a certain positive constant r represent real points of the surface ; this fact constitutes what the author calls the *Schönheitsfehler* of the pseudosphere as a representation of hyperbolic geometry. It is overcome by including among the " proper " points those imaginary points of the surface whose projections on the base-plane are real. With these taken into consideration it is further shown that the proper points can be related to the proper points of Cayley's representation in terms of an absolute conic in the projective plane.

A geodesic circle on a surface may be defined either as the locus of a point at a constant geodesic distance from a centre, or as a curve having a constant geodesic curvature. These definitions in general describe different curves ; only on the sphere, pseudosphere and other surfaces of constant curvature are the circles according to the former definition included among the circles under the latter. In the map of the pseudosphere a system of geodesic circles appears as a coaxal system of euclidean circles having the x-axis for radical axis, with a limiting point representing their common centre in case there is a centre ; the interpretation of the points of the map for which $y<r$ is then available for dealing with cases where there is no real centre. The geodesic circles are drawn from the map, and several particular cases discussed. Moreover the property of constant geodesic curvature leads naturally to consideration of the developable which touches the surface along a geodesic circle. A reader has to be interested to the extent of fifty pages in the form of this developable and in drawing its edge. When the developable is unwrapped on a plane the geodesic circle becomes a circle on the plane, and the surface may thus be considered as built up of a series of strips each cut out along the circles in the plane. Much stress is laid throughout the book on the actual form of the curves and their construction on a model ; certainly one of the most attractive features of the book is the figures which are so numbered and arranged as to be expressive even to a casual reader. There is also a beautiful plate showing photographs of geodesics and geodesic circles on an actual model of the surface. P. F.

Series de Fourier et classes quasi-analytiques de Fonctions. By S. Mandelbrojt. Pp. viii, 158. 35 fr. 1935. Monographes sur la Théorie des Fonctions. (Gauthier-Villars)

A fundamental property of a function of a complex variable, differentiable

throughout a domain, is that it is uniquely determined throughout the domain by its value at a single point together with the set of all its derivatives at the same point. For a function of a real variable, indefinitely differentiable throughout an interval, this is false, since the derivatives at a point only influence the function in the immediate neighbourhood of the point. However, certain classes of functions of a real variable have the property that each member is uniquely determined by its value and the set of its derivatives at a point. Such a class is called *quasi-analytic*.

Now a necessary and sufficient condition that a function $f(x)$ of a real variable should be expansible in a Taylor series about every point of a closed interval is that $|f^{(n)}(x)/n!|^{1/n}$ should be uniformly bounded throughout the interval. It follows in particular that the class of functions for which $|f^{(n)}(x)/n!|^{1/n}$ is uniformly bounded throughout a closed interval is quasi-analytic. The following question now arises: *if m_n is a sequence tending to infinity with n, and C_{m_n} is the class of all functions for which*

$$\underset{n\geqslant 1,\, x \subset (a,b)}{\text{upper bound}} |f^{(n)}(x)/m_n|^{1/n} \tag{i}$$

is finite, is every function of the class C_{m_n} uniquely determined throughout (a, b) by its value at a point and the set of all its derivatives there? In other words, is the class C_{m_n} quasi-analytic?

This problem was proposed by Hadamard, and a partial solution was obtained by Denjoy, who showed that a sufficient condition for 'quasi-analyticity' is that the series $\Sigma(1/m_n)^{1/n}$ should diverge. The first necessary and sufficient condition was given by Carleman, who obtained a condition which may be expressed in several forms. The one most closely connected with Denjoy's condition is *the divergence of $\Sigma\alpha_n$* where $\alpha_n = \max\limits_{\nu\geqslant 0} (1/m_{n+\nu})^{1/n+\nu}$, *i.e.* α_n is the least steadily decreasing sequence which is never less than $(1/m_n)^{1/n}$. Typical examples are $m_n = n!\,, n^n, (n \log n)^n$. The condition is also equivalent to *the divergence of the integral*

$$\int_1^\infty \frac{\log T(r)\, dr}{r^2}, \tag{ii}$$

where $T(r) = \max\limits_{n\geqslant 1} r^m/m_n$, or, alternatively, $T(r) = \sum\limits_0^\infty (r^n/m_n)^p$, where $p>0$. For example, if $m_n = n!$, in the first case we have $T(r) = r^{[r]}/[r]!$, so that $\log T(r) \sim r$, while in the second case, if $p=1$, $T(r) = e^r$, so that $\log T(r) = r$. In both cases the integral plainly diverges.

A full account of the solution of the above problem by the methods of integral functions is given in Chapter V of Professor Mandelbrojt's new monograph in the Borel series. The early chapters are devoted to introductory matter. Chapter I contains a brief survey of the Lebesgue integral. Chapters II and III contain some properties of Fourier series to be used in the sequel, and Chapter IV some properties of integral functions. These chapters are useful for reference, but cannot be recommended as a first introduction to the topics discussed. The later chapters contain results whose exposition the author describes as the "essential aim of the book."

In Chapter VI the methods of integral functions are applied to the following problem concerning Fourier series. *If a_n and b_n are the Fourier coefficients of a function which is periodic and indefinitely differentiable, and if $|a_n|$ and $|b_n|$ are less than $q(n)$, what are the conditions to be satisfied by $q(n)$ in order that the function should be uniquely determined by its value at a point and the set of all its derivatives there?* This problem was proposed and partly

solved by de la Vallée Poussin. Since all the derivatives are periodic the problem only concerns Fourier series for which a_n and b_n are $O(n^{-\Delta})$, for every Δ, and for these series $q(t)$ may always be chosen so that $Q(t)=tq'(t)/q(t)$ tends to $-\infty$ as t tends to ∞. The author supposes $q(t)$ to be of this form, and further, that $Q(t)$ is *steadily decreasing*, but it has been pointed out by Hille (in a review of a paper by the author *) that these conditions need not be used if one employs the Paley-Wiener technique of Fourier transforms, provided it be assumed that $q(t)$ is of *integrable square*. The author's conditions are, however, satisfied by a very wide class of series. The necessary and sufficient condition for quasi-analyticity in either case is the divergence of the integral (ii) with $T(r)=q(r)$.

In the last four chapters the author develops these ideas further, and also considers analogous problems in which the knowledge of the set of all the derivatives of a function at a point is replaced by the knowledge or partial knowledge of the function *throughout an interval* or *in the neighbourhood of a point*. In particular, some interesting results are obtained concerning lacunary Fourier series, and applications are made to problems concerning the singularities of Taylor series.

The results obtained are evidently not all in their final form, and this should prove a stimulant to those interested in the subject. The book is published in the usual cheap French binding, but is written throughout in a readable style and contains very few misprints. L. S. B.

Correspondence and papers of Edmond Halley. Arranged and edited by E. F. MacPike. Pp. xiv, 300. 21s. net. 1932. (Oxford)

Edmond Halley is known to most people in connection with a comet, and as having had something to do with the publication of Newton's *Principia*; but knowledge of his extraordinarily varied career is less widespread. While still at Oxford he formed the design of undertaking observations of the stars of the southern hemisphere, and with financial help from his father, the support of Sir Joseph Williamson and Sir Jonas Moore, and the recommendation of Charles II to the East India Company, who gave him a free passage in the *Unity*, he set out in November 1676 for the island of St. Helena. He was back again in England in May 1678, when Sir Jonas Moore reported to the Royal Society † "that he had completed his design by having taken the true places of above four hundred considerable stars." On 30th November 1678 he was elected a Fellow of the Royal Society; in the spring of the next year he was off again, this time to visit Hevelius at Dantzig and confer with him about a catalogue of the fixed stars. After making the Grand Tour of France and Italy in 1681, in company with Robert Nelson, later known for his theological writings, he married and settled down at Islington, where he fitted up a small observatory and began a regular course of observations of the moon. It was in 1684 that he first visited Newton at Cambridge, and asked him the famous question about the orbit described by a planet under the inverse square law. The story of how this led to the writing of the *Principia*, how Halley edited it, financed its publication, and saw it through the press, has often been told.‡ Meanwhile, on 27th January 1685/6, he had been appointed assistant secretary of the Royal Society, and for the next seven years was editor of the Transactions. His own contributions are most varied in character, dealing with such matters as the theory of equations, geometrical optics, astronomical

* *Zentralblatt*, 12 (1935), p. 65.

† Birch, *History of the Royal Society*, 3, 409.

‡ See, for instance, More, *Isaac Newton* (1934), 299 ff.

observations, meteorology, chronology, and "the ancient state of the City of Palmyra". In 1696, when Newton became Warden of the Mint, Halley was installed as comptroller of the local mint at Chester; an account of his troubles with the workmen there is given by More.* Two years later Halley was chosen to lead the first scientific expedition sent out by the Government, to investigate the variation of the compass in the Atlantic Ocean; he was appointed in August 1698 commander of the *Paramoor Pink* and set out on 24th October. He returned to England in September 1700, and his chart of the variation was published in 1701. After being employed on a tide survey of the English Channel he was sent out in 1702 and 1703 on two commissions to view and assist in fortifying the Emperor's seaports on the Dalmatian coast. In 1704 he succeeded Wallis as Savilian Professor of Geometry at Oxford, and for the next ten years we find him busy with the duties of his Chair, lecturing and publishing editions of Apollonius from the Greek and Arabic versions, with his own conjectural reconstruction of the missing books. From 1714 to 1719 he was again editor of the Philosophical Transactions. In 1720 he succeeded Flamsteed as Astronomer Royal, retaining his professorship; he added considerably to the equipment of Greenwich Observatory and made continuous observations of the moon between January 1722 and December 1739. The last few years of his life were troubled by ill health; he died 14th January 1741/2, aged 85, and was buried in the parish church of Lee, near Greenwich.

Mr. MacPike has put us greatly in his debt by recalling our attention to this remarkable man. His book, published at the expense of the History of Science Society, is not a life of Halley, but is a valuable collection of sources for the life. He begins with an unpublished contemporary memoir, of which the manuscript was until recently, at any rate, in the library of the Ratcliffe Observatory at Oxford. The MS. is anonymous, but Mr. MacPike gives good reasons for supposing that it is by Martin Folkes, President of the Royal Society from 1741 to 1752: the author was certainly a Cambridge man, who was up while Henry Sike (or Sykes) was Regius Professor of Hebrew (1705-12), which agrees with Folkes's dates (admitted to Clare 1706, matriculated 1709).

Then come Halley's letters, printed, as far as possible, from the originals or from copies preserved in the Royal Society Letter Book; the letters to Newton, which were printed by Brewster, have not been reprinted, which is a pity, since Brewster's transcripts are often inaccurate. A few letters from Hevelius and others to Halley are given in an appendix from transcripts in the Bibliothèque Nationale at Paris. In addition there is a valuable chronological list of Halley's correspondence, printed and unprinted. There follows a section of unpublished papers by Halley from the archives of the Royal Society. A voluminous appendix contains extracts from the Journal Books and Council Minutes of the Royal Society—Birch's *History* unfortunately stops at 1687—miscellaneous genealogical matter and a list of Halley's published writings, arranged chronologically but not bibliographically complete, nor at all easy for reference. The index is sadly inadequate, at least as far as names are concerned; a not very detailed examination has shown that there is no mention of Horrox, De Moivre, Street, Waller, Wren, for instance, though their names occur in the text, and that the references under Flamsteed, Gregory, Keill, Wallis are far from complete.

Mr. MacPike's book is a welcome sign of the revival of interest in the lives and works of the scientists and mathematicians of the seventeenth century, a revival of which other signs are More's life of Newton and the series of volumes of Robert Hooke's diaries and tracts published by R. T. Gunther.

F. Puryer White.

* More, *loc. cit.*, 448-450.

Projective Geometry. By L. N. G. Filon. Fourth edition. Pp. xviii, 407. 16s. 1935. (Arnold)

" We have in the plane a special line, the line infinity; and on this line two special (imaginary) points, the circular points at infinity. A geometrical theorem has either no relation to the special line and points, and it is then *descriptive*; or it has a relation to them, and it is then *metrical*."

It is notorious that many timid students have been prevented by the terror inspired by this sentence from ever reading further in the great classic of which it is the somewhat abrupt opening. Those who embark upon Professor Filon's work need fear no such shock; their danger indeed is quite of a contrary kind, less disturbing though perhaps more grave—that, namely, of never discovering at all what the difference is between a metrical and a descriptive theorem. It is indeed never made clear at any stage what Projective Geometry is; an unfortunate omission, if only in view of the title of the book. This curious vagueness is apparent from quite near the beginning, where the metrical definition of a cross-ratio is given, and it is then pointed out, as a rather lucky phenomenon, that this quantity is unaltered by projection; and startles one especially later, where the principle of duality is made to appear as a consequence of the possibility of reciprocating with regard to a conic, *i.e.*, of the properties of poles and polars. All very well; the book is elementary, and this is the familiar elementary approach—yes, but the author is not content to be elementary in the results he reaches. He tells the student, in one way or another, almost all there is to know about conics, as well as an enormous lot about quadrics, metrical properties and all; he even manages to include formulae about radii of curvature and torsion, in a chapter on " Projective methods in three dimensions ". He is in fact concerned with giving his reader the projective method (or a method involving as great a proportion of projective machinery as possible) of solving almost any problem that can be set him, rather than with teaching him projective geometry; he has given us a text-book for the training of wranglers, rather than of mathematicians. For this purpose, the book is probably as good as it could be; the omission of all talk about axioms of incidence, continuity, the algebra of casts, and all the fundamentals of projective geometry, while it may damn the student's soul, will doubtless win his heart; and the projective proofs given are on the whole very much more readable than those in the more theologically impeccable classics, such as Reye, or the newer work of Juel.

All this of course ought rather to have been said about the first than the fourth edition. (Indeed, it may have been said; I myself in 1908 was not only not writing, but not reading, reviews of mathematical books.) In excuse for saying it now, I can only plead that I had not the opportunity before.

As to the alterations since the third edition, all seems to be for the good. The book is now of standard size, and excellently printed. Many of the figures have been replaced by others, and all seem to have been redrawn and greatly clarified. Much matter has been added, especially an account of inversion in the plane, and a lot more genuine projective theorems about quadrics, including something on their linear systems, and on the apolar covariants of two quadrics. (It is odd, by the way, that, in this part of the book, the figure of eight points lying by fours in twelve planes is treated merely as analogous to the complete quadrangle, as of course in a rather artificial sense it is, but is nowhere said to be a set of eight associated points, though this more general figure is also discussed.) The order of the chapters in the middle of the book has been altered, probably with some gain in clarity.

It must be commented on that Professor Filon's terminology is in some respects peculiar to himself; the most confusing instance perhaps is " web ",

which here means the reciprocal of a net of quadrics; a word is perhaps wanted for the reciprocal of a linear system in general; but I should think "web" is too well established as meaning a linear system of dimension 3 to serve a new purpose. PATRICK DU VAL.

Elements of Statistics. With applications to economic data. By H. T. DAVIS and W. F. C. NELSON. Pp. xii, 424. 17s. 6d. 1935. Principia Press, Bloomington, Indiana. (Williams and Norgate)

This is an imposing, clearly printed and elegantly bound volume of 335 pages of text, 30 of appendices and 44 of tables. Written by a mathematician and an economist, members of the Cowles Commission for Research in Economics, the book professes to be an introductory manual of statistical methodology, carrying the student as far as a limited mathematical equipment will allow, and barring the door rigidly to further progress for a student not equipped with the calculus. The authors begin with the preliminary analysis of statistical data, introducing the frequency distribution, and with graphical analysis and elementary curve-fitting, anticipating here points of theory which are referred to more explicitly later on. After dealing with averages, they pass on to index numbers, and the analysis of time series. There is now a new starting point in a discussion of probability, leading to the binomial, normal and "skew-normal" distributions. A chapter on curve-fitting follows, in connection with which an elaborate set of tables is provided. The remainder of the book deals with correlation, simple, partial and multiple, with a brief final chapter on various types of statistical series. The tables provided are of logarithms, the exponential series, squares, square roots and reciprocals, the error function, a new table for testing goodness of fit, and tables for curve-fitting. The appendices consist of biographical notes on early mathematical economists, and hints on the use of logarithms and on interpolation and numerical integration.

Although the book is informative, there are points in which it falls short of an acceptable present-day standard. This is nowhere more evident than in the treatment of probable errors, especially in connection with the various correlation coefficients. The only reference to a more up-to-date treatment is contained in a footnote conveying a warning of R. A. Fisher against the misleading character of the standard error deduced from a small sample, but nothing positive is suggested in its place. There are many other points in which the book can be criticised. The frequent fitting of continuous frequency data by the binomial series, which is a series of discrete terms, has no mathematical justification. Incidentally, there is a strange statement here that the binomial *theorem* is the expansion of $(a+b)^n$, while the binomial *series* is obtained by putting $a=1$, $b=x$! There is a confusing of binomial with normal distributions, and of ordinates with frequencies, which is a little trying. It may be doubted whether the extra term introduced into the usual Stirling approximation of the binomial series to allow for asymmetry, leading to what is called the skew-normal curve, has any justification. In fitting this curve the authors have an unusual case in which the totals of observed and calculated frequencies are necessarily not equal, so that it may be doubted whether it is a fit at all. In connection with goodness of fit, the statement that theory limits the application of the method to data that are nearly normal is wrong, as is the whole application of the method, from the ignoring of an outlying frequency to the neglect of degrees of freedom. One wonders why it has been found necessary to expand Elderton's table of χ^2 to 10 decimal places with only the same range of values as the original table, so that it is quite impossible to interpolate to anything like this degree of accuracy. The method described of fitting a straight line, and even a parabola, to equidistant data

by means of 10-decimal-place tables is unnecessarily cumbrous. Not only for the elementary student, but for *any* type of student, much simpler methods are available. The long and involved proof of the probable error of the mean is not in the least needed. A few lines would be enough. The book will hardly satisfy the expert statistician, and it may be doubted whether it is on the right lines for initiating the beginner into the subtleties of an intricate subject of study. J. W.

Mechanics. By A. H. G. Palmer and K. S. Snell. Pp. xiv, 335. 15s. 1935. (University of London Press)

This book, which is intended for the use of higher forms in schools and first year students at the Universities, covers a field which is well defined by tradition and has been thoroughly explored by many writers. Little that is novel is therefore to be expected.

The standard is somewhat higher than would be expected from the description given by the authors. The majority of University students will find in it practically all the mechanics they will require in their pass degree course, for such topics as catenaries, virtual work, motion of a lamina, and orbital motion are included. Some preliminary knowledge of the subject is assumed and from the beginning calculus is used. This makes possible the introduction of a number of interesting problems which otherwise would have to be deferred, but it is open to question whether it is not better at this stage to separate the difficulties of mechanics from those of calculus. For the University student the treatment given is excellent.

Welcome features of the book are the insistence on graphical methods for both statical and dynamical problems and the stress laid on the dimensions of quantities. More emphasis on frictional forces and less on limiting friction might have been desirable, although in this respect this book is better than many. Reading between the lines one almost suspects that the authors would have preferred to have used absolute units throughout. There is an introductory chapter on vectors, and considerable use of these is made in the later bookwork.

Any textbook on Mechanics must consist to a large extent of examples worked and unworked. The worked examples in this book give the impression of being rather hard and complicated, and for the sake of the weaker student more easier ones might have been included with advantage. On the other hand there is an excellent collection of nearly 1200 unworked examples. Most of these are real mechanical problems and not exercises in trigonometry and calculus.

The book is well printed and illustrated with diagrams, although some of these are rather small. In one case of forces in equilibrium, the force and the side of the triangle which represents it are obviously not parallel. In the chapter on the dynamics of a lamina the authors have usually drawn two figures, one showing mass-accelerations and the other forces. It is strange that this excellent practice was not followed earlier in the book.

It is a pity that a little more care was not taken by the authors in writing out the manuscript. The appearance of many of the pages is spoiled by the use of the vinculum in connection with the square root sign when nothing is required, as in such an expression as $\surd 2$, or when a bracket is more suitable, as in $\surd(1+n^2)$; and much spacing is upset by the failure to use a solidus for a simple fraction, such as k/c, or a special type, as in $\frac{1}{2}g$. A study of such a pamphlet as *Notes on the Preparation of Mathematical Papers*, issued by the London Mathematical Society, would lead to an improved appearance and possibly a reduced price.

A few slips have been noted. When two bars of a jointed light framework cross it is not necessary to assume that they should be regarded as four separate bars; this is easily proved. The paragraph on the energy test of stability is far from convincing, and in that on the compound pendulum a rather bad omission is the precise definition of the moment of inertia—although the notation does suggest it. But as a whole the book is good and can be recommended confidently. R. O. S.

Einführung in die Zahlentheorie. By H. W. E. Jung. Pp. viii, 105. Geh. RM. 4.20; geb. RM. 5. 1935. (Jänecke, Leipzig)

This little book forms an adequate, and in some ways an attractive introduction to the Theory of Numbers. It seems likely, however, that most readers will skip the greater part of the first six pages. We then come to the "fundamental theorem of arithmetic", where one might have expected the author to explain very fully and carefully. Instead the theorem is taken at a rush, after which we slow down again to meet systems of rests (or remainders) to a given modulus. At this stage the author introduces "Additionstabellen", "Multiplikationstabellen", etc.—a device which appears to me very cumbersome, and which obscures rather than elucidates the simpler processes. From this point onward these tables (or arrays) are used in almost every proof. If any student wished to reproduce these proofs in an examination, extra large sheets of paper would be necessary. To many readers another irritation would probably be found in the fact that the author discards the familiar notation "$7 \equiv 3$, (mod. 4)" and uses instead "Nach dem Modul 4, $7 = 3$". This is longer, and is less easy to follow when the phrase "Nach dem Modul" is hidden in the middle of a lengthy explanation.

Nevertheless if the reader manages to overcome his aversion to the notation employed there is much that is well worth reading. The complete novice will undoubtedly appreciate the numerous worked examples, and even those who profess more knowledge of the subject may find them illuminating. One can hardly close a discussion of this book without congratulating the printer on his work throughout, and more especially on the reproduction of the wonderful diagram concerned with the theory of quadratic reciprocity. G. K. S.

A first Course in Differential Equations. By Norman Miller. Pp. 148. 7s. 6d. 1935. (Oxford)

Elementary Differential Equations. By L. M. Kells. Second Edition. Pp. xii, 248. 1935. 12s. (McGraw-Hill)

The transatlantic flow of elementary books on differential equations continues with singular regularity, with the slight difference that one of the immigrants on this occasion is a Canadian. Professor Miller's book is, like all of its kind, designed for students fresh from a first course in the calculus, and in a hurry to learn how to solve as many different types of differential equation as they can. Like most books of its kind, too, it has a first chapter which purports to explain what a differential equation is, and what one means by a general or particular solution, but whose accuracy is general rather than particular, as is shown by the statement (p. 5): "For a first-order differential equation a solution which contains an arbitrary constant is called the *general solution* or *primitive*." For the rest, the treatment of ordinary differential equations is thoroughly competent; a student who troubles to work through the numerous examples provided will no doubt acquire that specialised cunning needful for tackling similar problems in an examination paper. The chapter on linear equations with constant coefficients is in some respects much more instructive than many other treatments of that subject, but the author wastes far more

space in dodging the relation $e^{ix}=\cos x+i\sin x$ than he would use in proving it, as he could very briefly have done on the basis of assumptions already made. As to partial differential equations, it is very doubtful if the " object of indicating the possibilities of the subject and stimulating interest in its further study " can be attained by integrating the Lagrange linear equation, sketching the Charpit method, and touching on the fringe of the linear equation with constant coefficients. The beauty of partial differential equations lies underneath the skin, and is unseen by the superficial glance. Nor can its contemplation be enjoyed through the medium of the English language; the book that would tell of its fascination will not be written in this generation, for no author would gain by the writing or publisher by the sale.

The book of Associate-professor Kells seems to be designed for readers who have a very sound grounding in the calculus and in plane differential geometry, but whose knowledge of elementary algebra is perhaps scanty. The author is one of those who believe in translating results into long verbal statements which, no doubt, the student will memorise. Thus we have (p. 94): " *Rule. To find the particular solution* y_p*: (a) write the variable parts of the terms of the right-hand member X together with the variable parts of any terms that may be got by differentiating them* . . ." ten lines of it. Unfortunately such rules, even when correct as far as they go, may have exceptions—this particular instance has to be bolstered up by a 9-line supplement. Even so, the next page confesses that these rules " generally fail to give a solution " except in favourable circumstances, and there follows a verbose statement of the method of variation of parameters running to 14 breathless lines. The mathematician may find interest, and relief, in the technical problems that are used as illustrations. The theoretical part is really bad; the so-called existence theorem is misleading and the treatment of linear differential equations of the first order depends on fundamental assumptions made without explanation or attempt at justification. The exposition is not skilful, and is made to appear worse than it is by unintelligent typography. This is not a book that we can welcome over here; still less can we take to our bosom that revolting unit of mass, the *slug*.

E. L. I.

The case against Arithmetic. By E. M. Renwick. Pp. viii, 167. 5s. 1935. (Simpkin Marshall)

The title of this book is a little misleading. Miss Renwick has not made out any case against arithmetic, nor indeed has she attempted to do so. Her quarrel is with official views as to the amount of arithmetic which a child of ordinary abilities may be expected to have mastered by the age of eleven. The official views are those of the Consultative Committee on the Primary School as given in their *Report* (H.M. Stationery Office, 1931; 2s. 6d.).

Miss Renwick writes as a teacher of 25 years' experience and her book is an honest piece of work based on observation of actual children in the class-room. She has found in practice that it is quite often impossible as well as undesirable to teach arithmetic as mere mechanical computation. " We find," she says, " some children of nine years or more who are not happy in learning a mere technique; their intellectual needs remain unsatisfied."

If arithmetic is to be taught scientifically, as in her view it should be if it is to be of educational value, it seems to her unreasonable to expect children of eleven to have learnt to manipulate simple sums involving vulgar fractions, still more unreasonable to think that they should be able to multiply and divide simple decimals, and perhaps most unreasonable of all to ask them to make preliminary rough estimates of their work. Her remark, " It appears that ideas as to the nature and meaning of fractions need a considerable period in which to become definite and well-established " is supported by ample

evidence. With regard to decimals, she says, " Decimals, although easy to manipulate, offer more scope for unintelligent treatment than vulgar fractions. One might expect from the lateness of their date of invention that they would present more difficulties to children. This is actually the case . . . and to add to the child's troubles, they are usually introduced while vulgar fractions are still only half understood."

Her chapter on the difficulties experienced by young children in finding approximate answers is an especially good one. " The study of operations with approximate numbers is too difficult for children of eleven " is her very natural conclusion in view of her experience.

In reading the actual details of her teaching one feels that Miss Renwick sometimes labours difficult points unnecessarily with her pupils, and perhaps to some extent disheartens them in consequence. For example, a child can deal intelligently with $\frac{2}{3}$ of $\frac{4}{5}$ of a whole, and need not be puzzled by having to think that she is " multiplying " one fraction by another or that she is working out $\frac{2}{3}$ " times " $\frac{4}{5}$. From the child's point of view the work is a mixture of division and multiplication, and there is no need at first to teach otherwise. Again, if a child finds it unnatural to say that the ratio of A to B is 1 when the quantities A and B are of the same size, is it worth while bothering overmuch ?

In the preface to his *Algebra* Chrystal gives the following piece of advice to his readers : " Go on, but often return to strengthen your faith. When you come on a hard or dreary passage, pass it over ; and come back to it after you have seen its importance or found the need of it further on." *Mutatis mutandis* the same advice should doubtless be followed by the teacher. Nevertheless Miss Renwick has the root of the matter in her. In these democratic days it is more than ever important to discourage loose thinking and to do everything possible to produce clarity of thought and to develop the ability to distinguish between the essential and the detail. The author's sound convictions on these matters should be an encouragement to fellow-workers.

The Consultative Committee's *Report* recommends that greater prominence should be given in the mathematical lessons of the primary school to the study of geometrical form. It also expresses the view that it should be possible to effect a " very considerable reduction " of the time devoted to mathematics without loss of thoroughness. It would be interesting to have Miss Renwick's comments on these points also. It will, perhaps, be conceded that most of the training in exact thinking in the primary school is carried on by means of the arithmetical lessons. One-fifth of the available time of the curriculum does not seem an unduly generous allowance for such training. E. M. R.

The Teaching of Mathematics in the New Education. By N. K. Aiyangar, Trivandrum, S. India. Pp. vii, 420, v. Rs. 5. 1935. (To be obtained from the author)

This book is intended for the guidance of school teachers and educational authorities in India ; its interest for English readers lies more in the light it throws on Indian education than in any help it is likely to give to teachers in this country.

It is a formidable volume, divided into three sections, dealing respectively with the objects of learning mathematics, suitable subject-matter, and methods of teaching. The first section is far too diffuse ; the second indicates that Indian teaching has little contact with problems of everyday life ; the third, though it contains little if anything that is new or striking and omits much that is important, is the best part of the book. It is hard to understand how any discussion of British work can overlook the important summaries of accepted opinions of teachers in this country as expressed in the series of

recent valuable reports issued by the Mathematical Association; yet there appears to be no reference to these reports throughout the book.

If, however, the picture of Indian education here presented is correct, there is undoubtedly a great need for a book of this character; and it is possible that its length and tendency to repetition may not be distasteful to those to whom it is primarily addressed. C. H. O'D. A. and C. V. D.

An introduction to the theory of functions of a complex variable. By E. T. Copson. Pp. 448. 25s. 1935. (Oxford)

The first six chapters of this book deal with the classical properties of regular functions; the next two discuss developments which, in their method of treatment at any rate, belong essentially to this century, integral functions and conformal representation. Then we have the gamma function, hypergeometric functions, Legendre and Bessel functions, the elliptic functions of Weierstrass and Jacobi, and finally the elliptic modular functions leading round again to the modern ideas which have grown out of Picard's theorem that an integral function takes every finite value with at most one exception. The exposition is clear and easy to follow, and the author has provided us with a much-needed account of methods which are of the utmost value in modern function-theory—for example, the principle of the maximum modulus, and saddle-point integration—though we feel that more stress might have been laid on methods and less on functions; but this is possibly a matter of taste.

Professor Copson has based his book on courses of lectures given in the Universities of Edinburgh and St. Andrews; such courses must be as far as possible self-contained and this perhaps accounts for some fifty pages on convergence and uniform convergence, an amount of space which we could wish had been available at later stages in the book's progress. Even so, uniform convergence comes so late (Chapter V), that Chapter III has to include a rather dreary proof that the power series inside its circle of convergence has a sum which is a regular function.

In the first chapter, the complex number is defined * in a somewhat unusual way based on Peano's *Formulaire*, as the matrix

$$\begin{pmatrix} a & b \\ -b & a \end{pmatrix},$$

the definition being suggested by a consideration of transformations in the plane. This attractive idea has its dangers when multiplication is in question; the beginner might well suppose that any matrix product is commutative, while the more sophisticated student will avoid this trap but will not realise from the treatment given that the commutative property is anything more than an accident of the definition due to the speciality of the matrices involved; that it is the only commutative multiplication such that $z_1z_2=0$ implies that either $z_1=0$ or $z_2=0$ is nowhere apparent.

Cauchy's theorem is proved for a polygonal contour by Moore's method. It is then pointed out that the final step to the simple closed rectifiable contour is the really difficult one to make, and the point is met by reference to Pollard's memoirs. This seems beyond doubt the best way of dealing with the difficulty but would it not be possible to narrow the gap a little by discussing the case of circular arcs; pieces of straight lines and circles provide most of the contours used in the later parts of the book.

This volume will probably take its place as the standard English introduction to the subject. It is printed in the admirable style to which the Oxford Press has recently accustomed us. T. A. A. B.

* It should be added that the definition is not dependent on matrix ideas, but is developed in a self-contained fashion: the word is used here for brevity.

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